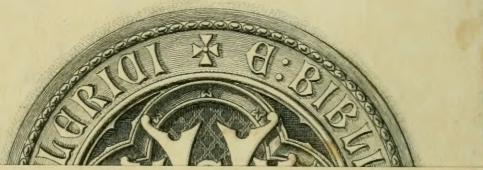

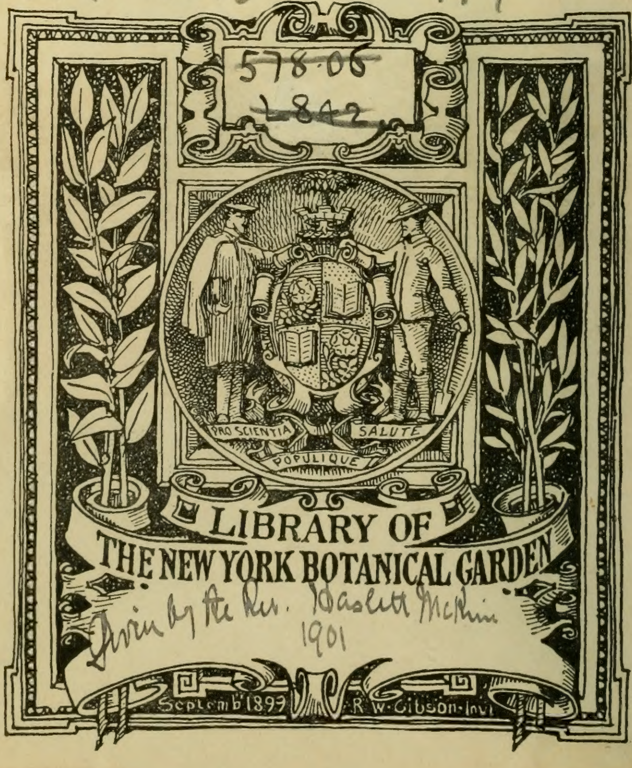
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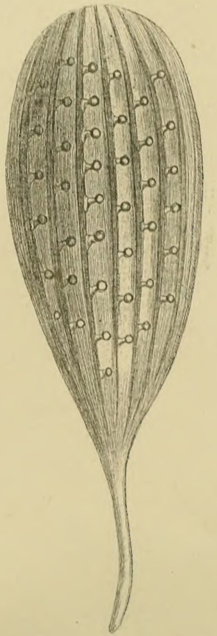
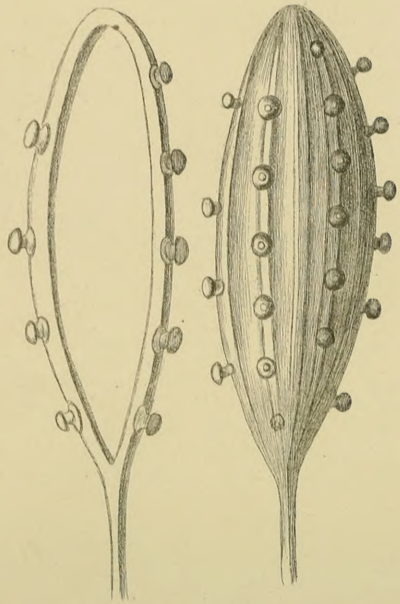
Fig. 1.



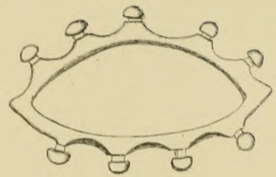
Front view.

Fig. 2.



Profile and Section of Profile.

Fig. 3.



Section at right angles to Axis of Scale.—

Tuffen West sc. W. West & C^o. imp.

Imaginary construction of Battledore Scale of Polyom: Alexis.
Fig. 1 nearly as observed under most perfect conditions.

THE MONTHLY MICROSCOPICAL JOURNAL:

TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

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THE

MONTHLY MICROSCOPICAL JOURNAL.

JANUARY 1, 1872.

I.—*The Markings on the Battledore Scales of some of the Lepidoptera.* By JOHN ANTHONY, M.D. Cantab., F.R.M.S., &c.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Dec.* 6, 1871.)

PLATES I. AND II.

HAVING long felt assured that the ordinary coarse representations of the markings on the "battledore scales" of butterflies were most imperfect, if not altogether erroneous, I last summer prepared a large number of slides of these scales, from the wings of the common meadow-blue butterfly (*Polyommatus Alexis* of Stainton), with a full determination to examine them critically, under varied conditions of light, and with the aid of the very best modern optical appliances.

I carried on this examination at intervals for several months, and the interest I felt in this self-imposed task increased greatly as I went on, inasmuch as, although I had been pretty familiar with the microscopic appearances of most of the scales of the Lepidoptera, I most certainly had never seen anything at all resembling what I made out on these battledore scales. In what I saw there was nothing incompatible with the usual representations of the markings, it was simply the reduction of chaos to order, though the clear, definite structures I made out were so singular, that I hesitated for some time to accept them as other than "Fata Morgana," of which the microscope is too often justly accused. However, a more and more careful elimination of all probable sources of error, as from spectra, shadows from obliquity of light, &c., showing me constantly the same definite appearances, I feel a confidence in the

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EXPLANATION OF PLATES I. AND II.

PLATE I.

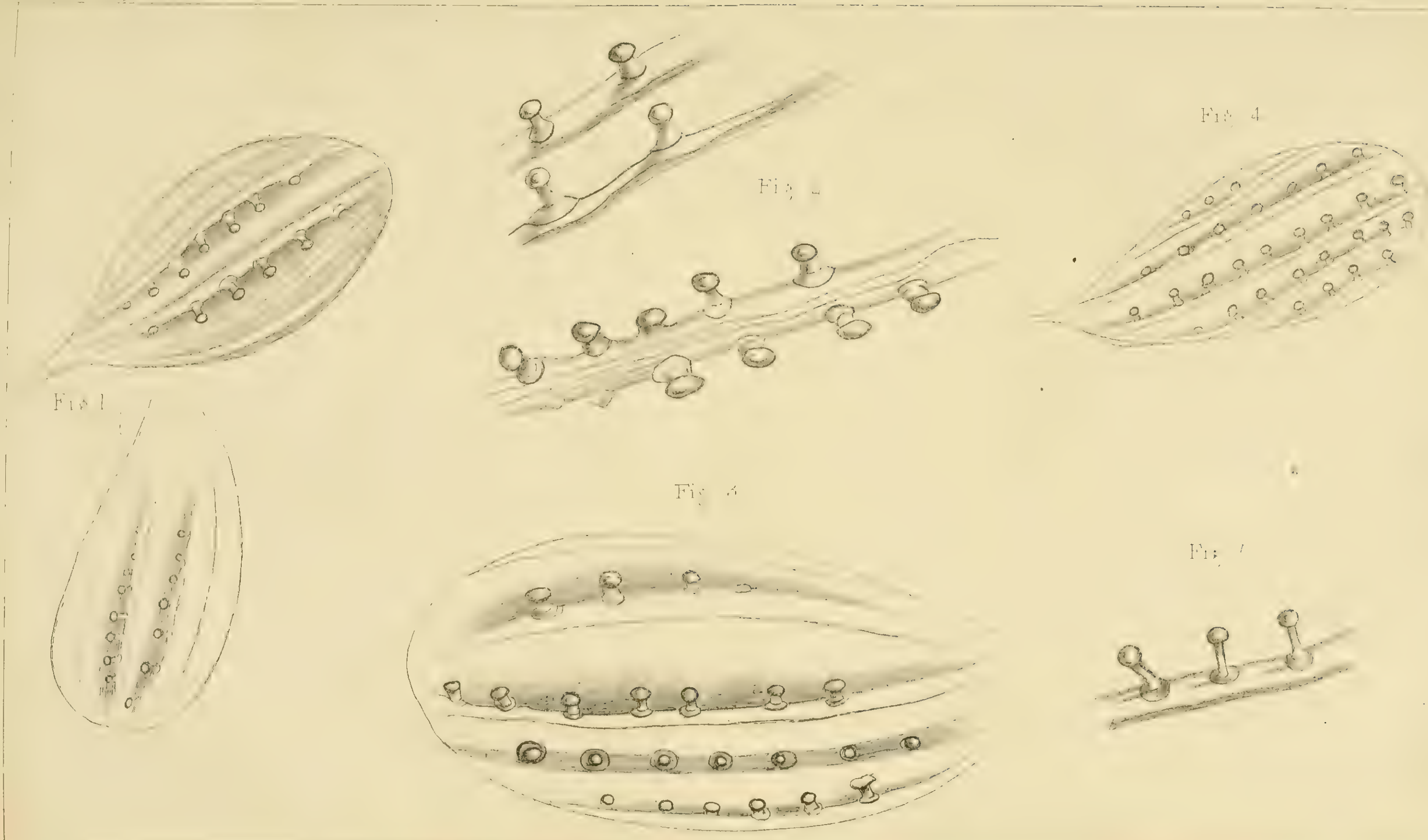
Imaginary Construction of Battledore Scale of *Polyom. Alexis* :—
Fig. 1.—Front view.
„ 2.—Profile and section of profile.
„ 3.—Section at right angles to axis of scale.

PLATE II.

Details of Battledore Scales of *P. Alexis.*

reality of what I observed, which causes me to bring the matter before the notice of my brother microscopists.

Let me state then, that I see the markings on the ribs of the battledore scales of the "meadow-blue" butterfly as elevations, very much resembling in shape the vegetable glands seen on the petal of the Anagallis—a well-known microscopic object; that is, the elevations have a base, a column, and a rounded head, or capital, as I have represented in the drawings on Plate II., and which drawings were carefully made at the time of observation from objects which can be referred to again by means of "Maltwood's finder." On Plate I. I have given an imaginary construction of the battledore scales, with sections, and which are intended to show, what I believe to be the arrangement of these columnar elevations on the ribs of the scales. Placed as these columns are, perpendicular to the plane of the scale, when they are looked at with the scales lying flat on the glass slide, head, column, and base being in the same axial direction, and in different planes, the appearance is much as it is usually represented, a dot upon a flatter dot, and of course more or less indistinct. But if a large number of scales be looked over carefully, some of them will be sure to be found slightly tilted over to one side, and others from accidental causes slightly bent or distorted, and it is on these scales that the elegant little columns show most distinctly, being seen more or less in profile. Supposing that a promising scale has been found, it requires, in order to see these markings well, that the illumination should be carefully managed, so as to avoid all obliquity, and the consequent production of heavy shadows, for *the purer and more direct the light*, the better I have always found true structure to be shown. To get this purity—if I may be allowed to call it so—I always work with a rectangular prism in the place of the usual plane mirror, and I am careful to place this prism, with its axis strictly at right angles to the direction of the illuminating ray, and I do this more readily by always placing my microscope facing the light, and having a small curtain just in front of the eye-piece. I should not do this except I found an advantage in it; and in this position the markings on the battledore scales seemed to me to be more clearly made out when the scale was in a vertical, or nearly vertical, position on the stage of the microscope. For very perfect vision, light should be most carefully centred, and the proper sized diaphragm stops employed, to cut off all "milkiness" and "glare" from the object, and then compensation should be very exactly made for the refraction of the covering glass, even though this nice operation should be facilitated by the use of the "immersion" principle. When a good scale has been selected, and these delicate adjustments attended to, I think the appearances I have de-



Tuffen West sc. W. West & C.o imp.

Details of Battledore Scales of Polyom. Alexis: *Drawn with $\frac{1}{8}$ & $\frac{1}{12}$ Objectives.*

scribed will be readily made out, particularly as they do not require at all a high power for a general view of them. The modern $\frac{1}{8}$th of Powell and Lealand, which is a superb objective, shows these small columns distinctly as I have drawn them in Fig. 5 of Plate II., but they can be well seen with a Beck's $\frac{1}{5}$th, which is so good as to bear a D eye-piece most satisfactorily. I wish it to be understood that I am not advocating the use of low-power objectives and high-power oculars in a general way, but now and then, as in the objects in question. Such an arrangement has its advantages, inasmuch as the low-power objective allows us to see a greater number of planes in focus, and so to get the general shape and relative position of an object which we can afterwards examine in detail, with all the fine definition obtainable by a deep objective and shallow ocular.

I hope I have not been wearisome in the description of these columnal elevations in the ribs of the battledore scales. The examination may seem trivial—time apparently thrown away upon an insignificant object, but the investigation has a value in this, if in nothing else, as showing that we must not take for granted that we know all about a structure, because it presents certain appearances when viewed in one position. Also, we must not lose sight of the fact, as we re-observe our well-known objects with our magnificent modern appliances, that, though our glasses are vastly improved, yet they are still far from perfect, and that probably those who come after us in examining these same objects, may give a half-pitying smile at what we thought we saw, and still more at what we failed to see.

II.—*The Nerves of Capillary Vessels and their Probable Action in Health and Disease.* By Dr. Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.

(*Read before the* Royal Microscopical Society, *Dec.* 6, 1871.)

Part I.—Anatomical Investigation.

Plates III., IV., and V.

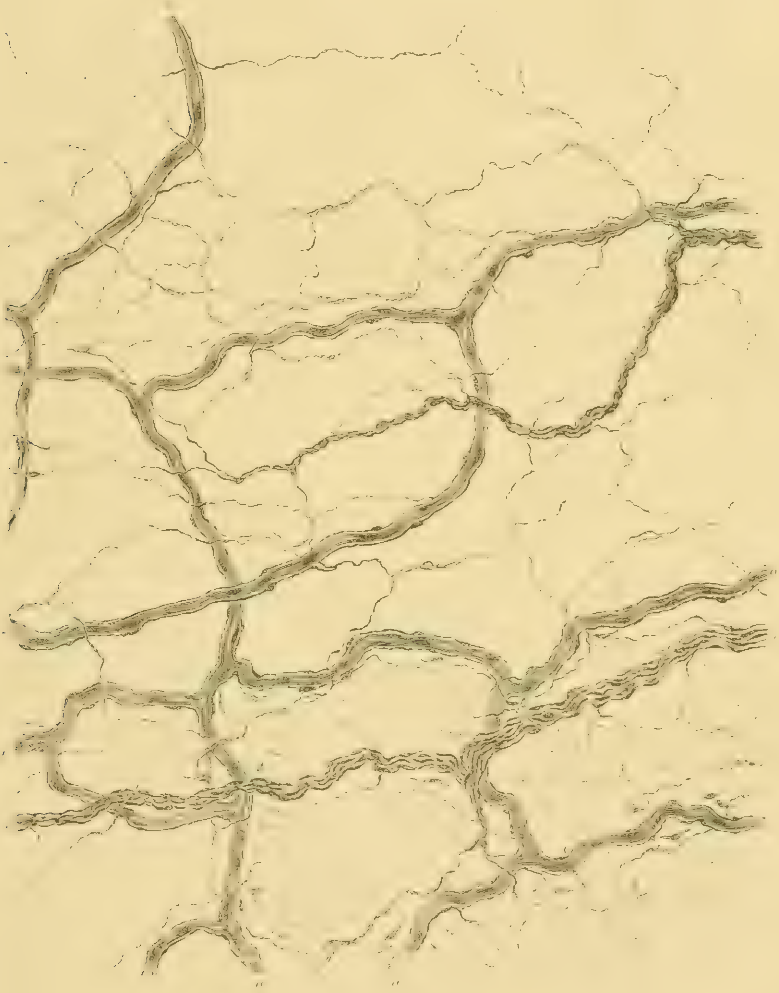
When studying the distribution of nerves to striped muscle in 1860,* I had seen nerves lying close to capillary vessels, and during the next two years obtained several preparations from the frog, toad, and newt, in which pale delicate nerve fibres, which had been followed from trunks containing dark-bordered nerve fibres, were discovered running by the side of almost every capillary vessel ramifying over an extensive area of tissue. In 1863 some drawings were published,† and in my Croonian lecture to the Fellows of the Royal Society in May, 1865, the distribution of nerves to capillary vessels was briefly described, and the function performed by them discussed. Although I have incidentally referred to the fact in several lectures and papers published since this period, I have not collected my observations, nor until now have I entered into the matter so fully as it deserves. The subject appears, however, to have been almost completely passed over by other anatomical observers in this country and on the Continent.‡

* "On the Distribution of Nerves to the Elementary Fibres of Striped Muscle," 'Phil. Trans.,' June, 1860.

† "On the Structure and Formation of the so-called Apolar, Unipolar, and Bipolar Nerve Cells of the Frog." May 7, 1863. 'Phil. Trans.': Part II., 1863. Published separately. Churchill, 1864.

‡ It is not a little remarkable that the opinion should, until very recently, have been entertained that *all* the arteries even are not supplied by nerves, and that the contraction of the unstriped muscular fibre is induced independently of nervous influence. It is some years since I was led to the conclusion that all forms of muscle are supplied by nerve fibres, and I am of opinion that every small artery possesses nervous supply even in those instances in which I have myself failed up to this time to demonstrate nerves. I do not think, however, that all capillaries have nerve fibres distributed to them, and it is quite certain that, as in the case of different tissues, the number of nerve fibres over a given area varies greatly. Kölliker made the remarkable observation that some of the arteries are destitute of nerves, and that the walls of arteries are not in such need of nerves as is usually supposed. Eberth, in Stricker's 'Handbook,' just published, says, "with the *exception of the capillaries*" the presence of nerves has been demonstrated in (upon ? L. S. B.) all vessels, but he remarks that he has not been able to convince himself of the precise mode in which they terminate, especially as regards the muscular fibres of the arteries and veins.

After this paper had been read on December 6th, Dr. Klein showed me some plates to illustrate a memoir by him upon the same subject, which will appear in the January number of the 'Quarterly Journal of Microscopical Science.' His specimens were prepared by the gold process, and give a different idea of the distribution of the nerves to that afforded by mine, some of which are ten years old. My own conclusions on the ultimate distribution of nerve fibres were formed several years ago, at a time when terminal nerve networks were denied in



Connective tissue covering part of the mylohyoid muscle of the frog, and extending from its posterior portion, showing capillaries and nerve-fibres distributed to them. *a*, Capillary vessels, with their nerve-fibres. *b*, Bundles of fine dark bordered nerve-fibres, from which fine nerve-fibres may be traced to the capillaries, as well as to their distribution in the connective tissue, where they form networks of exceedingly fine compound fibres. The engraving represents the specimen as if magnified only 110 diameters, but the original drawing was taken from it when placed under a much higher power. The specimen was mounted in 1863, and still (1871) demonstrates the points represented in the drawing.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

Structure of Capillaries.

It is scarcely possible to consider this question of the distribution of nerves to capillaries without referring to the structure of the capillaries themselves. It is curious to note the remarkable vacillations of doctrine with regard to some of the simplest matters of fact. Thus it has been lately affirmed by Eberth, in his article in Stricker's 'Anatomy,' that the walls of capillary vessels are *contractile* and composed of *protoplasm.* Now the evidence advanced in favour of this view is most defective—indeed, as regards the capillary vessels of the adult, there is none. No one has seen the membranous part of the wall of an adult or old capillary vessel contract, while it would be difficult to pick out a tissue less like protoplasm than the transparent material composing the capillary wall. If the thin elastic membrane of the capillary is to be called protoplasm, why should not the posterior elastic lamina of the cornea, or the thin transparent elastic membrane lining an artery be also regarded as of this nature? But these things, and living growing moving protoplasm, are quite different in their properties. It is indeed difficult to conceive how anyone who had really studied the matter, could speak of the oval "nuclei" or bioplasts of the capillary wall and the tissue of the wall itself which intervenes between them, and was formed by them, as being composed of the same substance "protoplasm;" unless he admitted that this wonderful material might, for example, constitute the moving, changing, semi-fluid sarcode of the amœba, as well as the firm, passive, unchanging material of which the yellow elastic tissue consists. In that case we should be applying the same name to things distinct in their nature and properties from one another, and we shall mislead ourselves and others by endeavouring to establish resemblances which do not exist in nature, and by ignoring differences which are observable by everyone who will simply examine with care and without preconceived theory. If the oval nuclei are protoplasm, and the membranous wall is protoplasm, the student will of course ask us how we account for the important differences between these two things, and then we shall be reduced to the unfortunate expedient of suggesting that one is protoplasm, and the other protoplasm "modified." If the inquirer has the bad manners to press us further and ask "how modified?" the only

Germany, and when it was supposed that only in a few exceptional cases did the axis cylinder of a nerve extend beyond the white substance. Not only are my networks of pale nucleated nerve-fibres now accepted, but it is maintained that much finer networks of nerve fibres ramifying upon and amongst epithelial cells and other elementary parts, and even upon an individual mass of bioplasm (nucleus), have been demonstrated. At present, however, I cannot regard the observations upon which it is thought to establish this view, more conclusive than those which a few years since led many to the conclusion that the axis cylinder sprang from the nucleus or nucleolus of the central nerve cell.

philosophical answer to give is "variously." This we shall find perfectly convincing, and not followed by any further troublesome inquiries concerning the nature of the physical nuclei or basis of capillaries.

Nuclei, or Masses of Bioplasm of Capillaries.

More or less connected with the wall of the capillary vessel are numerous "nuclei," consisting of *living matter* or *bioplasm*, which vary greatly in number in the capillaries of different tissues. Of these there are at least four distinct sets which may be distinguished in well-prepared specimens.

1. Those in the capillary wall itself, which have taken part in its formation, and which are intimately concerned in nutrition as long as blood circulates through the vessel. These vary greatly in number in different capillaries, as represented in many of my drawings, and in size also at different times. In some specimens they are extremely numerous, and, as I pointed out in 1863, project into the interior of the capillary vessel. Probably from these are detached particles of bioplasm, which pass into the blood current and may grow into white blood corpuscles.

2. Masses of bioplasm outside, but at a varying distance from the capillary wall with which they are connected by extensions or processes. These have no doubt originated from the first by fission, and are indeed an early stage in the formation of new capillaries, as may be proved by examination of the capillaries of adipose and some other tissues which undergo great and rapid changes even in the adult.

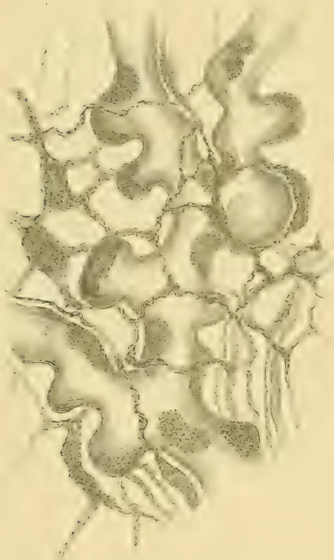
3. Oval masses of bioplasm, generally, but not invariably, smaller than the above, and, unlike them, sometimes crossing the vessel obliquely. These are connected with the delicate nerve fibres distributed to the capillary vessels. In some cases these bioplasts, as well as the fine nerve fibres connected with them, are almost embedded in the wall of the capillary, but oftentimes they are seen to be separated from it by a distinct interval, which varies much in extent, as is well demonstrated in some of my specimens.

4. Elongated and stellate masses of bioplasm which belong to the connective tissue. These vary greatly in number, size, and appearance in different tissues.

Nerve Fibres.

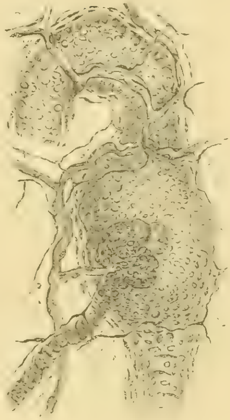
I have demonstrated that nerve fibres are distributed to capillary vessels in almost all the tissues of the frog and newt. Among these I would particularly mention the *skin* and *mucous membranes*, the capillaries of the *lung* and *kidney*, those of the *pericardium* and *fibrous membrane near the liver*, and those of the *mesentery*, as well as the capillaries of *muscle and nerve*.

Fig. 1.



Capillary vessel with peculiar diverticula. Palate of frog. Opposite *a* the terminal portion of a fine dark bordered nerve-fibre is seen, and all over the field numerous branches of the terminal net work of very fine pale nucleated nerve-fibres are represented. × 700.

Fig. .



Malpighian body and tube of the newt's kidney, showing fine nerve-fibres ramifying on the uriniferous tube and on capillary vessels. × 150

Fig. 3.

From an interval between the fibres of the mylohyoid muscle of the hyla. *a*, Trunks of fine dark-bordered nerve-fibres, with fine fibres coming from them, some of which may be traced to the capillary, while others are distributed to the muscular fibres, which are not represented in the drawing. The arrangement of the nerves supplying the capillary vessel is well seen. From a specimen nearly ten years old. × 215.

Fig. 4.

Fine nerves distributed to a capillary. Bladder, hyla. Showing also the bioplasts or "nuclei" of the nerve and those of the capillary vessel × 700.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

The nerves distributed to capillary vessels are much more difficult to demonstrate in mammalia, but I am sure that they exist, and in considerable numbers. The tissues of man and the larger mammalia are very unfavourable for so delicate an investigation, in consequence of the very diaphonous character of their nerve fibres and the great density of the connective tissue in which they are embedded, but in the mouse, shrew, mole, and some other small animals, they may be distinctly seen in very thin preparations. But during the last few years I have obtained some most excellent preparations from the bat's wing. In these, nerves to the capillaries may be demonstrated conclusively with the aid of a $\frac{1}{12}$th. The $\frac{1}{25}$th brings them out still more clearly. In the preparation I have with me, which should be placed under the twelfth of an inch object-glass, many delicate nerve fibres can be seen with great distinctness running very close to some of the capillary vessels. In order to demonstrate this fact, it is necessary to remove the dark cuticular covering from both surfaces of the membrane of the wing—by no means an easy operation or always followed by success.

Arrangement of the Nerve Fibres distributed to the Capillaries.

With regard to the general arrangement of these delicate nerve fibres, it is to be remarked that in many instances a fibre may be seen running on each side of a capillary vessel. The two fibres are often connected by short branches which pass over or under the vessel. Plate III., Plate IV., Fig. 3.

Not unfrequently the nerve is so close to the capillary that it cannot be seen distinctly in all parts of its course, but oftentimes the capillary shrinks after death, and then a distinct interval is left between its walls and the nerve fibre (Plate IV., Fig. 4). In some cases the nerves are still more numerous, and in the bat's wing I have seen three or four very fine fibres ramifying over a capillary for a short distance. Over the capillary vessels of the mucous membrane of the frog's palate (Plate IV., Fig. 1), these fine nerve fibres are easily demonstrated, and oftentimes may be seen a complete plexus of delicate nerve fibres with numerous oval and triangular masses of bioplasm connected with them. Upon the capillary loop of the fungiform papillæ of the human tongue (young subject) I have seen very fine nerve fibres in considerable number. Over the capillaries of the ciliary processes of the eye fine nerve fibres ramify very freely. All these nerve fibres are connected with oval masses of bioplasm, which vary in size and number in different animals and in different tissues of the same animal. In some cases the bioplasts are separated by a considerable distance from one another ($\frac{1}{50}$th of an inch), but often they are not more than $\frac{1}{500}$th of an inch apart. At the point where a fine branch divides into two others the mass of bioplasm is triangular;

and specimens in which four, or even five excessively delicate nerve fibres diverge from a mass of bioplasm with as many angles is occasionally met with. Thus, as in other situations, lax networks are formed, the meshes of which are for the most part long and narrow.

Central Origin and Peripheral Connections of Nerve Fibres distributed to the Capillaries.

As regards the origin and connections of nerve fibres ramifying upon the capillary vessels I have some important facts to record.

1. In many instances, particularly in the fibrous membrane about the bladder of the frog, I have followed fine nerve fibres from ganglion cells to the smallest arteries, where they form a plexus from which pass branches direct to the capillaries.

2. I have traced nerves direct from the ganglia embedded in connective fibrous tissue to the capillaries. See Plate V., Fig. 3.

3. A specimen under the microscope this evening proves that a fine nerve fibre given off from a bundle of dark-bordered fibres distributed to voluntary muscle may be followed direct to a capillary. See Plate IV., Fig. 3.

4. From the ganglia between the muscular and mucous coats of the small intestine of any small animal (mouse, mole) fine nerve fibres can be followed in considerable numbers, and traced to the capillaries of the mucous membrane. I have never been able to see them on the vessels of the villi, but feel convinced they are to be demonstrated as far as this point. Even in the human subject I have succeeded in making some good, but not perfectly demonstrative specimens.

5. "I have seen a dark-bordered nerve fibre divide into two branches, one of which ramified upon an adjacent vessel, while the other was distributed to the elementary fibres of the muscle." Also "nerves distributed to arteries and to elementary fibres of striped muscle have been seen to be derived from the same trunk of dark-bordered fibres."*

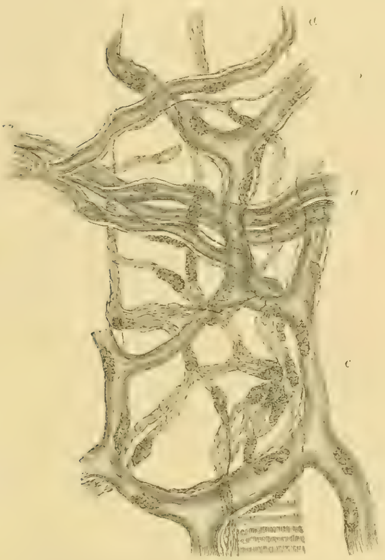
With reference to the peripheral connections of nerves distributed to capillaries, I have to remark—

1. That in the papillæ of the frog's tongue, I have followed fibres from the expansion of the sensitive nerves above the capillary loop to the capillary vessels, and also from a somewhat similar structure in the mucous membrane of the snake's mouth to the capillaries. In both cases fibres are also given off from the bundles of dark-bordered fibres before they break up to form the reticulated sensitive expansion.

2. The bundles of sensitive fibres which break up and form expanded networks in the meshes of the beautiful capillary net-

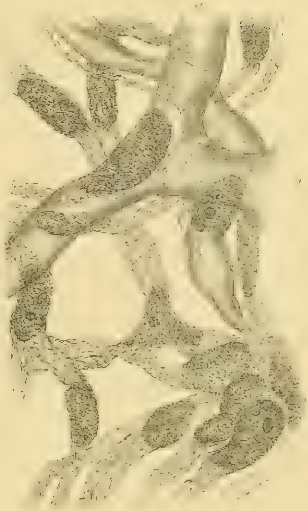
* Croonian Lect., 'Proceedings of the Royal Society,' May 11th, 1865.

Fig. 1.



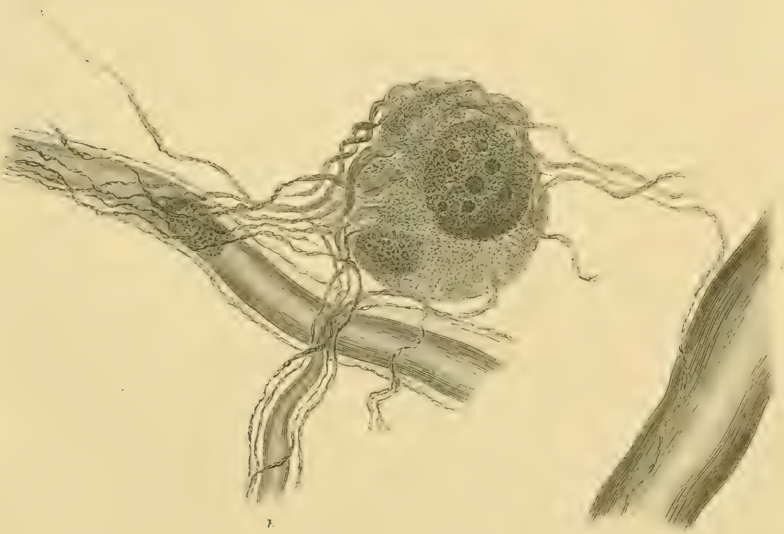
Portion of an elementary muscular fibre from one of the abdominal muscles of the white mouse, with dark bordered fibres (*a*) crossing over its surface. It will be observed that a fine fibre, which seems at the upper part of the figure to be one of the outlines of the tubular membrane of one of the dark-bordered fibres, leaves the dark-bordered fibre and passes to the capillary vessel ***b***. I have so often observed a dark-bordered nerve-fibre distributed to muscle divide into two branches, one of which passed to a vessel, while the other ramified upon the muscle, that I believe the arrangement to be usual and probably essential.

Fig. 2.



A small portion of Fig 1, to the left of letter ***c***, much more highly magnified. A portion of the capillary vessel is marked ***a***. The capillaries, dark-bordered nerve-fibres, and pale nucleated fibres represented in the drawing, as well as the intervening connective tissue, can be completely stripped off from the surface of the sarcolemma without tearing that membrane. Of the structures therefore represented in this drawing not one can lie beneath the sarcolemma. 1865

Fig. 3.



Ganglion cell with fibres connected with it passing off in three different directions. The large dark-bordered fibre immediately below the cell has no connection with it, and is probably not influenced by it. One of the fibres passing from the cell becomes a fine dark-bordered fibre (*b*). *a* is a capillary vessel. From the bladder of the hyla. × 1,800. December, 1862.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

work at the extremity of the mole's nose give off fibres which may be traced to adjacent capillaries.

3. Branches from the nerve fibres ramifying over the minute arteries may be frequently followed from these to the capillary vessels.

4. In the case of the striped muscles of the chameleon's tongue, I have succeeded in following a fine fibre from the so-called nerve tuft to the neighbouring capillary vessels.

5. In the muscular coat of the frog's bladder, in that of the oviduct, and in the muscular coat of the small intestine, the fine nerves form an intricate interlacement, some fibres of which are distributed to the muscles, while others ramify upon the little arteries, veins, and capillaries.

These nerve fibres distributed to the finest capillaries of many tissues have therefore been traced from *ganglia*, from *sensitive and motor nerve trunks,* from the *peripheral ramifications both of sensitive and motor nerves,* and they are intimately related to the ultimate ramifications of some of the *nerves of special sense.*

These anatomical facts suggest many considerations bearing upon the mode of action of the peripheral portion of nerve fibres, but the subject is too extensive to discuss in this paper. It may form the subject of a separate memoir.

Method of Demonstration.—I have already described the method pursued in the preparation of the specimens. It is that which I have followed for more than ten years, and which in my hands has been most successful.* I feel sure that it is capable of further improvement in practical details, and that, upon the principles which I have laid down, delicate structures, which have not yet been seen by man, will be demonstrated by patient and well-practised observers. The process is troublesome, and for this reason it has not been in much favour. In these days investigation must be conducted with such haste, and new facts discovered so quickly, that there is little chance of getting many persons to spend sufficient time in mere practice to enable them to gain the requisite skill for the very much more minute investigation of the structure of the most delicate textures which is now so much required, and which must be carried out before we can hope to arrive at positive conclusions on fundamental anatomical questions of the greatest importance.

*** The probable mode of action of the nerves described in this communication will be considered in the Second Part of the memoir, which will be published in the next (February) number of the Journal.

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* 'How to Work with the Microscope.' 4th Edition. 'The Physiological Anatomy and Physiology of Man.' By Dr. Todd and Mr. Bowman. Second Edition. By Dr. Beale. Part I., page 57.

III.—*Note on Dr. Barnard's Remarks on "The Examination of Nobert's Nineteenth Band."* By J. J. Woodward, Assistant-Surgeon, U. S. Army.

My own paper "On the Use of the Nobert's Plate"* and Dr. Barnard's reply,† state so fully the arguments in favour of a complete count of the band on the one hand (my method), and on the other, of a count of a measured portion of the band only, say twenty to thirty lines (Dr. Barnard's method), that I think the question may now advantageously be left by both of us in the hands of those who are competent to judge of the comparative certainty and accuracy of the two methods.

Nevertheless I think justice to myself requires that I should call attention to the contradictory interpretation which my distinguished friend has been led to give to the same series of observations in consequence of what I must regard as his misplaced confidence in a method which I believe to be deficient in the accuracy required for this purpose.

Dr. Barnard's original observations were related by him in a private letter to Mr. Stodder, dated January 29, 1868, and Mr. Stodder published them in a note to his paper "On Nobert's Test-plate and Modern Microscopes" in the 'American Naturalist,' vol. ii. p. 93.‡ Mr. Stodder tells us that Dr. Barnard resolved the nineteenth band with a Spencer's $\frac{1}{12}$th and a Tolles's $\frac{1}{5}$th, *both dry lenses.* With the former objective he made a series of counts of a measured fraction of the band, which gave by reduction a series of estimates of the number of lines to the English inch, 106,226 lines being the smallest, and 115,474 lines being the largest estimate.

At the time Dr. Barnard made these observations the subject of the spurious lines so readily developed on the plate had not received due consideration. I had myself about the same time supposed that I had resolved the nineteenth band with a dry Wales's $\frac{1}{8}$th, but subsequently convinced myself that I had been deceived by the spurious lines.

The observations of Dr. Barnard recounted by Mr. Stodder as well as some made by Mr. Stodder himself with a Tolles's immersion $\frac{1}{6}$th, reported in the same paper, became therefore the subject of a private correspondence, and several conversations between Dr. Barnard and myself, and that gentleman became so thoroughly convinced that he also had been misled by spurious lines, that in a public lecture delivered before the American Institute in New York city, November 25, 1868, he announced his conviction that the

* This Journal, July, 1871, p. 26.
† Ibid., October, 1871, p. 194.
‡ Reprinted in the 'Quarterly Journal of Microscopical Science,' July, 1868.

finest lines of the nineteenth band plate "have never been as yet fairly resolved." *

In the following spring, however, having received an immersion $\frac{1}{16}$th from Messrs. Powell and Lealand, I was so fortunate as to effect undoubted resolution of the refractory bands, and to furnish a count of the number of lines; and now Dr. Barnard, as his recent paper shows, *reconsiders* his own judgment of his own work, and claims to have effected true resolution with the *dry* Spencer's $\frac{1}{12}$th, though he writes me he still admits the lines shown by the dry $\frac{1}{5}$th to have been spurious. I confess I am not convinced by his present reasoning that the judgment deliberately announced in his public lecture of November 25, 1868, was erroneous, and must continue to believe that a method which has permitted so careful and conscientious an observer to contradict his own conclusions in this manner, ought not to be preferred to the more accurate and absolute criterion of resolution which I have proposed for the higher bands of the plate.

IV.—*A New Erecting Arrangement, especially designed for Use with Binocular Microscopes.* By R. H. Ward, M.A., M.D., Professor of Botany and Microscopy in Rensselaer Polytechnic Institute.

For dissecting and other manipulations under magnifying powers, the simple microscope is awkward and unsatisfactory, and has been made to serve as a binocular only with very low powers; but the superb field of the compound microscope has been comparatively little used for these purposes, because few persons can work to advantage under an inverting arrangement; the erectors usually furnished are not good, and the use, otherwise satisfactory, of a good objective as an erector has not as yet afforded the advantage of binocular vision. The simple expedient now proposed is designed to increase the usefulness of the stereoscopic binoculars in ordinary use by rendering them easily available for purposes which require an erect image.

Last summer, the writer proposed, at the Indianopolis Meeting of the American Association, to place, for certain purposes, an erecting objective below instead of above the regular objective of the microscope. Then, of course, the regular objective becomes the erector, and the accessory one below acts as the objective. This simple expedient, applied to Wenham's and other non-erecting binoculars, leaves little to be desired for the purposes of a dissecting microscope. The lenses of a $1\frac{1}{2}$ or 2 inch objective (preferably a solid or single-combination one) may be packed or screwed into

* 'Annual Report of the American Institute of the City of New York for the year 1868,' No. IX., page 43.

the upper end of an adapter which, when screwed into the nose-piece of the microscope, carries them up close to the binocular prism, and into the lower end of which, lengthened more or less by two or three adapters of various lengths, the object-glass may be screwed. A more elegant but scarcely more satisfactory arrangement is an adapter with a sliding-tube adjustment, which varies to the extent of an inch or more the distance between the erector and objective. Different powers and distances will, of course, be used, according to the wants of different observers. The combination which has proved most convenient in my hands consists of a 2-inch erecting lens close to the binocular prism and a $\frac{2}{3}$-inch objective at a distance, measured to its lower end, of from 3 to $4\frac{1}{4}$ inches below the erector; giving powers of 10 to 50 diameters, and requiring a working distance between the stage and the binocular prism of $4\frac{1}{2}$ to 5 inches, which is quite practicable with large stands. A shorter working distance may be gained at a slight disadvantage. With a 2-inch erector and a $\frac{4}{10}$-inch objective, powers of 8 to 50 diameters can be secured without raising the binocular prism more than 4 inches above the stage; and with a 1-inch erector and $\frac{2}{3}$-inch objective a power of 40 diameters is obtained with the prism $3\frac{3}{4}$ inches from the stage. When, however, sufficient working distance cannot be obtained, the object may sometimes be placed upon the sub-stage, or oftener the sub-stage removed and the body racked down, so as to focus through the empty stage upon the table, a block or box, or an extemporized stage occupying the usual position of the mirror and illuminated by the mirror after the method suggested by Mr. James Smith. In this case it is often desirable to increase the working distance between the prism and the object by varying the lenses employed. Thus a $1\frac{1}{2}$-inch objective at from $3\frac{3}{4}$ to $5\frac{3}{4}$ inches from the erector will give powers of 6 to 50 diameters, and working distance from prism of 7 to 10 inches. The erector may also be removed an inch or more from the prism. When this latter arrangement is to be used exclusively, the apparatus is further simplified by screwing a 2-inch objective into the nose-piece in its usual position, as an erector, and screwing or sliding over it an adapter carrying a $1\frac{1}{2}$ or 2 inch objective from 4 to 6 inches lower down. Some contrivance is required to illuminate transparent objects under the lower powers; but opaque and translucent objects on a black ground can be dissected and manipulated with great facility.

The same erecting arrangement can be used in connection with monocular microscopes that have no draw-tube. It may also be used as a means of working Wenham's and other binoculars with high powers. With powers of 500 or 1000 diameters, however, it is still difficult to obtain good definition or to fully light both fields.

TROY, NEW YORK, U.S.A., *Nov.* 6, 1871.

V.—*On a New Micrometric Goniometer Eye-piece for the Microscope.* By J. P. Southworth.

After a few experiments by Dr. H. T. Porter and myself, we have succeeded in making an eye-piece micrometer and goniometer which equal in accuracy and surpass in simplicity and cheapness any we have seen, and we have used those of some of the best makers in this country. The objection to the eye-piece micrometers in use is the want of boldness in the division lines, which makes them fatiguing and hurtful to the eyes. To overcome this objection we were led to experiments in making micrometers by the aid of photography, which have resulted in success. The steps of the process are these:—

1st. A scale of 100 heavy India-ink lines of about $\frac{1}{6}$th of an inch apart is drawn on a dead-white surface of Bristol board. The lines marking every ten divisions are 6 inches long, and extend 1 inch each side of the scale; those marking every five divisions are 5 inches long and extend one-half inch beyond the scale; the remaining lines are 4 inches long.

2nd. By photographic process for copying engravings, a negative is taken, on which the scale equals about 2 inches in length, and is intensified by mercuric chloride and potassium cyanide.

3rd. With a copying camera and lens for taking transparent positives for the magic lantern, a transparent positive of this negative is taken on micrometer glass, reducing the scale to the length of one-half inch. In this the lines are $\frac{1}{200}$th of an inch apart. After intensifying, washing, and drying, a cover of thin glass is cemented on with Canada balsam, and the slide cut to fit the slit in the micrometer eye-piece. It can be also mounted with a spring and micrometer screw, like Jackson's micrometer. In our micrometer the lines appear to stand out in relief, and are jet black, while the spaces between them are translucent enough to admit of the accurate measurement of the details of minute algæ and fungi to the $\frac{1}{25000}$th of an inch.

Regarding the goniometer:—

1st. A circle about 18 inches in diameter is drawn with India-ink, divided into degrees. The centre is indicated by a dot, and one diameter is drawn. Every 5 and 10 degrees are indicated by longer lines than those indicating single degrees. Every 10 degrees of each quadrant are numbered from 0 to 90.

2nd. A negative 2 inches in diameter is taken by the process referred to above, and from this a transparent positive is taken on a circle of micrometer glass cut to fit the tube of the microscope. It is covered with a circle of thin glass cemented with balsam, and mounted to fit the tube at the focal point of a positive eye-piece. A cobweb is drawn across the diameter of the lower lens. When a

crystal is to be measured, the stage is moved till the apex of the angle coincides with the centre of the goniometer and the diameter with one side. The eye-piece is now turned till the cobweb crossing the diameter at the centre coincides with the other side of the angle. Now the number of degrees of the angle can be read at the circumference. The advantage of this over the ordinary microscopic goniometers is, that in ours the angles of the crystal and the degrees of the goniometer are on the same line of sight within the tube of the microscope, while in the ordinary goniometer the degrees are marked outside the tube. The photographic processes by which the above are made can be learned by consulting any of the standard works on photography, under the sections that treat of copying engravings and taking transparent positives.—*Silliman's American Journal, December*, 1871.

VI.—*On the Action of Hydrofluoric Acid on Glass viewed microscopically.* By H. F. Smith, Esq.

Having had occasion some time ago to use hydrofluoric acid (in solution) for the purpose of etching on glass, I observed that in some lights the varieties of colour presented while the action was going on were very brilliant. This led me to examine it microscopically. The hydrofluoric acid was prepared in the ordinary method, from calcium fluoride by the action of sulphuric acid. The solution was then diluted and kept in a lead bottle for use when required.

When the acid was first dropped upon the glass, no action was evident, the appearance presented being simply that of a drop of water on glass. In a very short time, however, the drop became a little duller, but this almost immediately cleared away, and several small particles, seemingly of glass, were seen floating in the drop. These seemed to be undergoing a process of *fusion;* the appearance being similar to that seen when a small portion of metal is thrown into some of the same substance in a state of fusion; it is tossed about for some time, and then finally disappears. This was what evidently appeared to me to be going on here, the hydrofluoric acid having apparently a solvent action on the glass. What strengthened this opinion was the presence of magnificent colours, changing every moment as these small portions of glass were liberated from the larger piece, and were undergoing the process of solution, thus leading one to suppose they consisted of small glass prisms, the colours being more perfect than those obtained by *water-prisms* simply. Some of these particles were completely surrounded by a halo of colour, as if they had been thrown into a

variegated solution. The principal colour evident in such cases was a deep green, but dark blue was also seen at rare intervals.

The above observations were repeated several times, and always with the same results, with the exception that the small particles of glass floating in the drop of acid *exploded* now and then, causing a great commotion in the liquid and throwing up little jets of finely-divided acid, behaving as if the small glass particles were hollow spheres. I may also mention that when these explosions occurred, bright flashes of light were visible, resembling closely the appearance of rainbows seen in waterfalls.

NEW BOOKS, WITH SHORT NOTICES.

Traité du Microscope, son Mode d'Emploi; ses Applications à l'Étude des Injections; à l'Anatomie humaine et comparée, etc., etc. Par Ch. Robin, Membre de l'Institut (Académie des Sciences) et de l'Académie de Médecine, etc. Paris. J. B. Baillière et fils. 1871.—Many works as we have in our language upon the microscope, and manifold as are the instruments described, the apparatus mentioned, and the tissues displayed, it is nevertheless a fact that there is in the English language no book which deals more with the subject of human anatomy than with those of the comparative branches of histology. Doubtless this is because those who devoted themselves to the subject of comparative anatomy were so much more numerous than the students of, let us say, human histology. Hence, doubtless, the reason why in such admirable works as those of Dr. Carpenter, Dr. Beale, and Mr. Hogg, much relating to the human microscopic anatomy has been omitted to make room for facts in the anatomy of Protozoa, Cœlenterata, Annulosa, and Mollusca. 'Tis true that Dr. Beale's work enters more upon human histology than either of the others, but then he does not go fully into the subject for the very natural reason that those who pursue it are vastly fewer than those who delight in the study of Rotifera and Infusoria. Of course we do not refer to Dr. Richardson's American work, for it deals with human anatomy almost exclusively, and furthermore it is small in extent of pages, and is almost devoid of illustration. There is therefore room for such a work as the present one, which attempts to deal with the whole of microscopy, and which does fair justice to each of the departments it takes in hand.

The work now before us is a large 8vo volume of more than 1000 pages, and with over 200 woodcuts, besides three or four plates at the end. It is divided into three parts. The first deals with the subject of injections; the second part treats of the various forms of microscope constructed in France, England, &c., &c.; next it deals with the instruments and apparatus to be employed in the production of cells and the general mounting of microscopic objects, the making of sections, the chemical agents employed in microscopy, the modes of using the microscope, and of reflecting the light from the mirror, &c. The third and last part, which forms about half the volume, was, we may say, almost absent from the work which appeared in 1849, and hence the present volume may be looked upon as a comparatively new treatise. It deals with the application of the microscope, and its auxiliaries to anatomy, physiology, medicine, animal and vegetable natural history, chemistry, and agricultural economy. It is this part of the volume which, in our opinion, gives it a special character, and helps to add to our information facts which do not exist in any of the English works already noticed.

The opening chapter deals with the subject of injection, and goes into the question very fully. The author gives a preference to Dr.

Beale's Prussian blue fluid, but he suggests some modifications which do not appear to us of any importance. After going very fully into the subject of the different forms of injection, the various fluids, the transparent and opaque, he enters on the subject of pursuing injection upon the living animal. He gives a section to the method, and describes fully the process employed by himself and M. Onimus. Then follow the precautions to be taken in injection, the differences between injection and infiltration, the rupture of parts during the operation, the injection of veins, partial injections, and last of all, of physiological injections.

Then comes the chapter on microscopes and apparatus. This is not so good as we should have expected. It is confined to French and English microscopes, and it figures none of the latter save a couple of the instruments of Ross. The chief illustrations it contains of advantage to English masters are those of the dissecting lens of Lacaze-Duthiers; the new great microscope of Nachet, which is a peculiar instrument; and a prismatic eye-piece, which reverses the position of the rays, and makes an object appear in its ordinary position. The rest of the chapter deals with the various methods of correction, and it gives a figure of the instrument for measuring the angle of aperture of objectives; it deals well and fully with its subject. It is followed by a chapter detailing at considerable length the various chemical agents employed in microscopical research. Then follow various chapters describing the drying of tissues, the preparation of microscopic objects, microscopic dissection, thin sections of hard tissues, the instruments employed in preparing the sections, the method of illumination, followed by a discussion upon the different modes of lighting up an object, and dealing tolerably fully with the mathematical bearings of the subject; and lastly there is a very short chapter which leaves unnoticed much of the work done on the subject of photographing microscopic objects, &c., &c.

Finally there comes the remaining half of the volume which is devoted to the methods of preparing specimens of the various tissues already named, for microscopic purposes. This is by far the most valuable and likewise the most novel part of the work. Of course the reader will find much in it which he has already learned from Dr. Carpenter, and Dr. Beale, but he will also find abundance of novel matter which we will venture to say he cannot discover elsewhere. Thus the book is on the whole a useful and valuable addition to our microscopic literature. It has only one fault, and that is the paucity of its illustrations. Dr. Carpenter's, which is really a smaller work, contains very much more than double the number of illustrations which M. Robin has thought necessary, thus rendering it most valuable to the student. We trust that M. Robin will see to this in next edition, and we hope that that period may soon come round, for the volume is really a most valuable addition to our microscopic literature.

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of the Mole's Snout.—This is given very fully in the last part of Max Schultze's 'Archiv,' in a paper by Herr Th. Eimer, of Würtzburg,* which is briefly abstracted in the 'Lancet.' The muzzle of the mole in part supplies the place of its extremely defective organ of vision. The sensory area is situated at its anterior extremity, and presents even to the naked eye a papillated surface. When examined with the microscope, the papillæ are found to be low elevations varying from a tenth to a fifth of a millimètre in height. The papillæ are composed of epithelium, which not only projects from the surface, but dips into the corium: the cells occupying the little fossa in the corium are ribbed cells. Each papilla is traversed from base to apex by an hourglass-shaped canal, the periphery of which is of course bounded by the epithelial cells. The canal is filled by a structureless homogeneous mass, probably of the nature of mucous tissue. The nerves traverse this tissue, and end by free extremities in the outer division of the hourglass-like cavity, which Eimer terms the tactile cone. The disposition of the nerves is very peculiar. If the skin of the snout be treated with chloride of gold, the corium is seen to be richly supplied with a plexus of medullated nerve-fibres. From these, small fasciculi containing twenty or more are given off, which run to the base of the papillæ and enter the inferior cone, at the same time losing their medullary layer. They then appear to arrange themselves in the form of a circle, presenting in transverse sections very much the appearance of the copper wires in the electric cables. As the fibres ascend the tactile cone towards the surface of the skin, they are connected by a slight nodal enlargement or varicosity with each successive tier of cells, terminating as near the surface as the third or fourth epithelial cell-layer. Here and there a nerve-fibre (cylinder axis) runs amongst the epithelial cells of the papillæ external to the cone. As the number of papillæ is about five thousand, and as about twenty nerves enter each tactile nerve-cone, the total number of fibres must amount to about one hundred thousand, and their very superficial termination is a matter of great interest.

Various Investigations on the Dust of the Air.—We have of late had various reports on this subject. The first is by Mr. J. Sidebotham, and was read before the Manchester Philosophical Society at a late meeting. It appears that while travelling by rail between Saltley and Camp Hill, he spread a paper on a seat of the carriage, near the open window, and collected the dust that fell upon it. A rough examination of this with the $\frac{2}{3}$rds power showed a large proportion of fragments of iron, and on applying a soft iron needle he found that many of them were highly magnetic. They were mostly long, thin, and straight, the largest being about $\frac{1}{50}$th of an inch, and, under the power used, had the appearance of a quantity of old nails. He then with a magnet separated the iron from the other particles. The weight alto-

* Band 7, Heft 3.

gether of the dust collected was 5·7 grains, and the proportion of those particles composed wholly or in part of iron was 2·9 grains, or more than one-half. The iron thus separated consisted chiefly of fused particles of dross or burned iron, like "clinkers"; many were more or less spherical, like those brought to our notice by Mr. Dancer from the flue of a furnace, but none so smooth; they were all more or less covered with spikes and excrescences, some having long tails like the old "Prince Rupert's" drops; there were also many small angular particles like cast iron, having crystalline structure. The other portion of the dust consisted largely of cinders, some very bright angular fragments of glass or quartz, a few bits of yellow metal, opaque white and spherical bodies, like those described by Mr. Dancer, grains of sand, a few bits of coal, &c. After the examination of this dust, he could easily understand why it had produced such irritation; the number of angular, pointed, and spiked pieces of iron, and the scoriæ or clinkers, being quite sufficient to account for the unpleasant effect. He thinks it probable that the magnetic strips of iron are laminæ from the rails and tires of the wheels, and the other iron particles portions of fused metal, either from the coal or from the furnace bars. The large proportion of iron found in the dust is probably owing to the metal being heavier than the ordinary dust, and accumulating in cuttings such as those between the two stations named. If he had to travel much by railway through that district, he should like to wear magnetic railway spectacles, and a magnetic respirator in dry weather.

American Researches on the Microscopy of the Atmosphere.—Mr. Charles Bailey drew the attention of the Manchester Philosophical Society to the subject *à propos* of the above paper by Mr. Sidebotham. He stated that Mr. Charles Stodder, of Boston, U.S., had been recently making a series of researches on the microscopic contents of the atmosphere of that city. Amongst other investigations he was led to examine a fine black dust from a beam in the polishing shop of the United States' Armoury, at Springfield. He found it to contain a few vegetable fibres, some apparently organic fragments, and some broken crystals; but the great mass of it was made up of amorphous fragments of iron, of the $\frac{1}{100}$ mm. and upwards in size, as well as curved and irregular fibres and masses of iron, with sharp jagged edges, from 5 to 15 mm. in size; there were also some very minute perfect spheres, probably iron. In trying the effect of the magnet upon this dust he found it removed it from a sheet of paper as completely as if it had been swept off with a brush, and he concluded that the non-metallic portions adhered to the iron particles by the thin layer of oil with which all the particles of dust were coated. To prevent this dust passing into the atmosphere of cities, Mr. Stodder recommended a plan which had been put in practice many years ago in this country, but abandoned from the indifference of the workpeople, *viz.* the fixing of magnets in the immediate neighbourhood of grindstones and polishing wheels. In the same report, Mr. Stodder alludes to the labours of two members of the Manchester Society—Dr. Angus Smith and Mr. J. B. Dancer—in examining the contents of the air, and points out an important matter considerably affecting the results of such investigations, *viz.*

the method employed for filtering the air through water. The usual method has been to place a small quantity of pure water in a large bottle, and shake it in the air under investigation, repeating the operations with renewed volumes of air in the same water; but Mr. Stodder shows how impossible it is to intercept all the foreign particles in the atmosphere in this way, inasmuch as the smallest bubbles of air which pass through the water very much exceed in size the particles of matter which are sought for, and myriads must elude observation. A greater difficulty, however, is to obtain absolutely pure water for such experiments; and whether filtered or distilled water was used, a drop evaporated on a glass slide always left a deposit of scaly and granular particles. This result, as Mr. Stodder justly says, puts an end to this mode of investigation, and throws a cloud of suspicion on all reported researches in this line, when water was the medium used.

Description of new Genera and Species of Australian Polyzoa.—Mr. P. H. Mac-Gillivray, M.A., read a paper on the above subject some time ago before the Royal Society of Victoria. Still, as the journal containing the paper only reached us a couple of months since, we think the subject may be new to some of our readers. In this paper are given descriptions of forty-eight species, including two genera, of Australian polyzoa, which cannot be satisfactorily referred to any of those hitherto described. The identification of polyzoa by the aid of descriptions alone, however accurate these may be, is often extremely difficult. The species here described, as well as the others existing in Victoria, will be figured in Professor M'Coy's 'Memoirs of the Museum,' where Mr. Mac-Gillivray hopes to be able to give descriptions of all those with which he is acquainted. Specimens will also be deposited in the National Museum. He has added a list which contains all the Victorian species he has in his collection, with the exception of a few not yet determined. The descriptions then follow, but are far too numerous for our space. See Reports of the Royal Society of Victoria.

The Tissues of Man and Apes.—Dr. Lionel Beale, F.R.S., imagines that there are material differences between the tissues of man and apes, and he calls on naturalists to investigate them. In a very able and lucid address to the Quekett Club, he says:—"It is wonderful what haphazard assertions are made in these days concerning the likeness or identity of dissimilar things. Observers, who should test these assertions, and ascertain whether they are accurate or not, permit them to pass without comment, and the public accepts them as literally true. We are told, for example, by Mr. Darwin, that it is 'scarcely possible to exaggerate the close correspondence in general structure, in the minute structure of the tissues between man and the higher animals, especially the anthropomorphous apes.' But Mr. Darwin does not tell us that he or anyone else has made the observations upon which the statement is founded. A careful comparison of the tissues of man with the corresponding tissues of apes in minute structure is much to be desired, but it has never been made, and it is quite premature to speak of the supposed 'close correspond-

ence' as if it had been proved to exist. As to the close correspondence in chemical composition, asserted to exist between man and apes, the same remarks may be made. Such correspondence has yet to be shown."

Vitality of Disease Germs.—In a recent number we referred to Dr. Grace Calvert's numerous experiments on this subject. Since they were made, Mr. H. B. Yardley writes as follows to the 'Chemical News':—"Dr. Calvert (in previous numbers) in his paper says, that at 400° germ life cannot exist, but I take it that his experiments were conducted at 100°, 200°, 300°, 400°, 500°, and 600° Fahr. only, and that the intermediate temperatures were not tried. If this is the case, it might happen that even at 330° Fahr. germ life would be destroyed, which will consequently render disinfection by heat practicable. Not having the time or apparatus to repeat Dr. Calvert's experiments myself, I take the liberty of mentioning this." We may mention that the letter was *à propos* of a statement by Mr. Richard Weaver in a former number of the Journal, that 350° is the highest heat that may be used for disinfecting without destroying the fabrics.

How to Destroy Disease Germs in Clothing.—In a letter to the 'Chemical News,' Mr. G. E. Davis states his own experience on this subject. *A propos* of Dr. Calvert's researches, he says he has devoted a considerable portion of time to the study of disinfectants—in fact, sanitary chemistry generally, and the fact struck him as soon as he entered the field that the miasm was not destroyed by the heat of most disinfecting chambers. In fact, in some, where the heat employed is very low, owing to a badly-constructed oven, very little good can accrue from the baking process alone. At the Sanatorium attached to Eton College the beds from the scarlatina patients used to be baked at rather a high temperature, but owing to scorchings having taken place the temperature has been considerably reduced. The heat now employed, if used alone, is, in his opinion, only just enough to warm the disease germ and make it feel very comfortable. The heat cannot be raised too high owing to its irregular working, the temperature being sometimes 40° or 50° higher than the thermometer indicates. In December, 1870, he found the heat was only got up to 120° Fahr., but it was supposed to rise to 160° Fahr. He gave as his opinion that the heat had very little action upon the disease germ, and proposed placing a vessel containing carbolic acid mixed with an equal volume of water in the hot chamber. He has now written to know whether the system proposed by him has been fully carried out. He finds that for the last nine months, when articles have been placed in the disinfecting chamber, the heat has been averaging from 120° Fahr. to 160° Fahr., and a vessel of dilute carbolic acid (1 to 1) has been placed in the next compartment to the clothes. The vapour of the phenol has been carried over by the combined effects of heat and steam, and has saturated the clothing. This he considers a good method, certainly better than baking alone, for ensuring complete destruction of any lower organism. Previous to December, 1870, the method of disinfecting the patients' linen before washing, was to steep in solution of bleaching powder, but owing to the solution being

sometimes made improperly, holes were *burnt* in the clothing: he thereupon proposed a dilute solution of carbolic acid; it is easy to mix, perfectly soluble, and has no action upon fabrics in the diluted state.

Nucleated Sporidia.—On this subject, of great interest to fungologists, Mr. M. C. Cooke gives a capital paper in the last number of the 'Quekett Club Journal.' If, he says, we take one of the cup-shaped fungi (*Peziza aurantia*, or *P. vesiculosa*) when mature, in its fresh state, and cut it down through the centre, we shall observe first that the outside and inside of the cup varies somewhat in texture, as well as in colour. That the inside has usually a smooth, delicate, waxy appearance, and, if exposed to the light for a few minutes, little smoky puffs of the minute spores will from time to time be ejected from the surface, as of miniature discharges of fairy artillery. By cutting a thin slice from the cut surface of the section and placing it under the microscope, a good notion of its general structure will be obtained. The slice should be as thin as a sharp knife and a steady hand can accomplish. If this slice be placed in a drop of water, or pure glycerine, on a slide, and covered with thin glass, then submitted to a slight pressure, it may be examined freely with a quarter-inch objective. Towards the inner surface, which is the *hymenium*, numerous transparent tubes, or cylindrical sacs, present their upper extremities, whilst their lower ends coalesce with the cellular substance of the cup. These cylindrical bodies are the *thecæ* or *asci*, mixed with thread-like filaments called *paraphyses*. Sometimes the paraphyses are simple, at others branched, and either attenuated or clavate at their tips. In a few cases the club-like extremities of the paraphyses are coloured, but usually the asci and their contents, as well as the paraphyses, are colourless. In this genus the paraphyses are important features in the determination of species, since they offer considerable variation in different species. What may be their special function has not been satisfactorily determined. Some authors have suggested that they may be barren asci; but this suggestion is far from confirmation by fact, no observations having yet traced the development of paraphyses into asci, or explained why they are so distinct from asci even in the earliest stages at which they can be traced. Similar bodies are also present in the cups of lichens. The asci in their earliest stages are filled with a granular matter, which ultimately is collected (normally) into eight spherical, elliptical, or elongated sporidia, which fill the ascus, and when mature are discharged by rupture at the apex, in little puffy clouds of sporidia, as already intimated.

NOTES AND MEMORANDA.

Death of M. Edouard Claparède.—Owing to the press of various matters for the past two or three months, the notice of the death of this eminent zoologist has been "crushed out" for want of space. His death occurred some time since at Sienna, at the early age of thirty-

nine years. His memoirs, begun in 1857, have been issued with remarkable rapidity. His principal works consist of his monographs on the Infusoria and Annelids. In all his papers, his thorough physiological and anatomical training is perceptible, his details being always discussed in all their general bearing. Living in Geneva, and a pupil of Johannes Müller, he wrote with equal facility French and German: an admirable draughtsman, his many papers, which have appeared in the principal German and French scientific periodicals, are excellently illustrated and wonderfully accurate. His qualities as an original and independent observer are best seen in his larger memoir on the Annelids of the Gulf of Naples, and his observations on the Anatomy and Embryology of the Invertebrates made on the coast of Normandy. His style was remarkably clear and his information very extensive, as is shown from his scientific reviews in the Archives de Genève. Thoroughly independent in his scientific opinions, he never allowed himself to be carried away by weight of authority, and no scientific charlatan protected by eminent names was allowed to pass current; his reviews and criticisms were often sharp, but always just, and never personal. The Academy of Geneva, where he was Professor of Anatomy, will find it difficult to fill the place of one who, in spite of his failing health, showed an enthusiasm for his science rarely equalled.

On two new Lamps for the Microscope.—These lamps, constructed by Mr. E. Richards, F.R.M.S., are designed for the purpose of those who are in the habit of taking their microscopes with them to their friends. The following description will make clear their properties. One is intended to burn benzoline, the other parafine. The receptacles are both of same size: the former is filled with a sponge, saturated and drained off in the usual way, and will burn for three hours; the other is filled likewise with parafine, and will burn for six hours. The stand gives a height of 12 inches, and weighs only 6 oz., and is arranged as follows:—Half of the stem screws into the burner of the lamp, thereby preventing any escape of the liquid or spirit; the other half screws through the bottom of the stand—forming a short pendulum in appearance—convenient for carriage. The lamp is enclosed in a neat case for the pocket. These lamps were exhibited at the last meeting of the Royal Microscopical Society, and gave great satisfaction.

Col. Horsley's Cylinder for Oblique Illumination.—The cylinder is $1\frac{1}{2}$ inch long, and $1\frac{1}{4}$ inch in diameter, made of brass tube silvered inside. The light is thrown on to it either directly, or from prism or bull's-eye used as a prism. A dark ground is easily got with low angle $\frac{1}{4}$-inch, and without trouble or loss of time (so say several witnesses) the hemispheres of Angulatum are shown very beautifully. The first apparatus was made by silvering the inside of the tube of the sub-stage which carried the polarizer; this was done by rubbing on it a solution of nitrate of silver, to which hyposulph of soda had been added. This silvers brass with a good surface, and moderately permanent, in a moment.—[This note was received from Dr. J. Miller, V.P.R.M.S.]

CORRESPONDENCE.

Butterfly Scales.

To the Editor of the 'Monthly Microscopical Journal.'

Brussels, *November* 22, 1871.

Sir,—I think it is most important for everyone that he should be able to see through the microscope all that is to be seen, and nothing else.

Having for some months past made a constant use of the paraboloid with immersion objectives of Ross and of Hartnack ($\frac{1}{12}$th Ross, Nos. 9 and 11 of Hartnack), and studied the scales of many butterflies and moths of European, Chinese, and Brazilian origin, I have been struck by the elegance of the beaded appearance those objects presented.

I may thus entirely confirm Mr. S. J. McIntire's views: *those scales have almost all of them a beaded appearance, perfectly defined;* but where I differ totally with this most distinguished Fellow of the Royal Microscopical Society is when he asserts in his paper "On the Minute Structure of the Scales of Certain Insects," read the 9th Nov., 1870, "that those beads have no real existence as *beads*, but are due to the interference of the rays of light by corrugated membranes; that they are, in fact, *ghost-beads*."

Reasoning upon this hypothesis, it is not at all wonderful that Mr. McIntire should have attributed his failures, when "he wished but could not call into existence *those almost palpable beads*," as he calls them himself, so distinctly they are visible, "to a predisposition of his mind, and to the want of that necessary adroitness in manipulating which everyone knows very well is not always at one's own command."

Be that as it may, it must be admitted that his whole reasoning on the matter in hand shows very well that there exists a very serious difficulty of making a proper use of high-power objectives, and of selecting that kind of illumination which is best adapted to the nature of the object which is to be seen, and in particular which is wanted to show the beaded appearance of the scales of the wings of insects.

But all those reasonings seem not to me to be conclusive. One assertion alone, if it were justified by facts, would be very serious; it is that these beads disappear altogether without leaving any traces or remains whatever, when the scale is crushed.

I should beg Mr. McIntire to reconsider this his last assertion, because I am positive that the beads subsist as distinct and isolated things after the scale has been crushed; but it may happen that the scales which he has seen were partially so crushed as to leave in the parts which were crushed no remains at all. Then, but then only, the assertion of Mr. McIntire seems very well justified by facts, but in that it is no conclusion at all.

Now for what I have observed, and the mode of illumination I have made use of.

I pass the light through a paraboloid, of Mr. Nachet's make, of Paris: this glass is similar to the paraboloid of the principal English opticians, but somewhat smaller in size.

When well centred, and in focus, the paraboloid gives a very white light with high power; and every object that comes under the eye is wonderfully clearly and perfectly illuminated; but if the object-glass is not well corrected for the thickness of the glass cover it will at once be apparent, because then you will have no perfect image at all, but an unseemly object, the markings of which will become the more apparent the more you approach the true correction to be made, and then you will see every particle of the object strikingly well defined, and every scale of many, if not of all insects, will appear covered with dots or beads.

I think it was due to an imperfect correction that Mr. McIntire was not always able to see the beaded appearance of the scales.

Here follows a list of butterflies or moths whose scales I have particularly observed, and which wear most clearly the beaded appearance, with nothing more to do to show them but a just correction of the object-glass:—*Grapholytha minutana*, *Pterophorus pterodactylus*, *Platyptilus pilosellæ*, *Alucita hexadactylus*, *Clecophora luctuosella*, *Harpella bractiella*, *Tortrix Hoffmanseggana*, *Vanesse Atalanta*, *Moro-sphinx*, *Crambus*, *Minophora metaxella*, *Vanesse gamma*, *Cosmophora composana*, *Sericosis urticana*, *Liparis chrysonhaca*, *Podalirius*, *Yponomerita malinella*, *Vanesse urticæ*, *Yponomerita padilla*, *Orthosia Gothica*, *Coccyn Buolèana*, *Colyade-Hyale*, *Apatura Iris*, *Agraulis vanillæ*, *Rhétenor*, *Deilephila Elpenor*, *Melanippe*, *Atlas*, *Morpho Helinor*, *Junonia Genoveva*, *Lycorea atorgatis*, *Alcandor*, *Erythonius*, and many more, which are either unknown to me, or whose names are of no scientific nomenclature to be here inserted.

In confirmation of the present letter, I beg the Royal Society to accept a dozen of my slides of butterflies' scales, some of them *selected*. They are of the *Melanippe* (Brazil), *Alcandor* (China), *Deilephila Elpenor*, *Rhetenor*, *Papilio agaris*, *Junonia Genoveva*, *Lycorea atergatis*, *Atlas*. I hope they will be graciously accepted.

Using the same mode of illumination with the paraboloid, I have tried to discover the real nature of the markings of the *Surirella gemma*, that most beautiful puzzle, and I have clearly seen the dots which this interesting diatom wears; they are similar to those of the *Pleurosigma Balticum*, and similarly disposed; that is to say, they make lines which intersect each other at right angles.

This appearance of the *Surirella gemma* must be the true one, because, having tried that same diatom with a double reflector obliquely disposed at right angles to each other, I have obtained the same appearance.

So I cannot sufficiently recommend the use of the paraboloid; it gives a beautiful dark-ground illumination with low power, and with high power a very white one, and its effect is more secure; I should say the true one, in opposition to every other mode of illumination, direct, oblique, with condensers or diaphragms.

Its effect is particularly striking if you observe the flow of the vegetable sap (rotary circulation in plants), as in the *Tradescantia*

virginica or *Campanula sylvestris;* the corpuscles which are seen floating in the fluid have the appearance of little spheres, so clearly defined that it seems impossible that human skill can ever contrive to make an optical instrument which will show more or better than our microscopes when well and judiciously employed; and yet it is evident that there is something more to be seen, because those spheres are not only dragged by the force of the stream in which they are immersed, but they have their own and individual motion; so they must have some sort of propellers as yet unknown and unseen.

I am, sir, yours truly,

CHEVALIER HUYTTENS DE CERBECQ.

THE ANGULAR APERTURE OF TOLLES' $\frac{1}{5}$TH.

To the Editor of the 'Monthly Microscopical Journal.'

BOSTON, MASS., *November* 15, 1871.

DEAR SIR,—Permit me to reply to the question of your correspondent B. in the November issue of your Journal. The angular aperture of the Tolles' objective ("nearly $\frac{1}{6}$th") used by Dr. Woodward as "immersion" varies from 140°, at "uncovered," to 170°. How Dr. W. used it I do not know. Used "dry" ($\frac{1}{5}$th) it ranges from 110° to 170°. With another front "system" it is a $\frac{1}{4}$th 110° up, and upwards. The aperture as well as the power increasing as the lenses are brought nearer together.

A few words may be added on B.'s remarks on aperture. He refers to the "confusion which exists in the minds of most microscopists about the classification of glasses," and conveys the idea, though he does not express it in words, that angular aperture is not taken into consideration. That such is the case with *some* makers, and *some owners* of microscopes, is undoubtedly true; but so far as my experience goes it is not the case with "*microscopists*." With them aperture is as much considered as focal length; and I find that with "microscopists," "focus and aperture are in fact both essential factors in the denomination of an object-glass." This subject was elaborated by Dr. W. B. Carpenter in the first edition of 'The Microscope and its Revelations,' published more than sixteen years ago—a work that is in the hands of most microscopists, and which B. has probably seen. Since that time, however, skilful opticians have made object-glasses of very high angular aperture and retaining greater penetrating power than such as were known to Dr. Carpenter then, and consequently better adapted for purposes requiring that quality than he probably expected they ever could be, and the same glasses retain the advantages derived from the great aperture. All opticians have not progressed in this direction, but some continue to make their lenses as they did twenty years ago,

As a trip to the United States is now a favourite one with Englishmen, I hope that B. will make us a visit. Americans have been reported as having "made some very good instruments," and it will afford

me pleasure to show B. objectives of large aperture with good penetration. Still the fact remains that objectives of very high aperture, if good, are different things from those of very low aperture; and when two glasses are to be compared the angular aperture should be taken into consideration.

It must also be remembered that glasses of great aperture, *if good*, are more costly than those of small aperture; because the proper corrections for sphericity and colour involve, and require, more skill, and a better knowledge of pure optics in the optician, and also more time and more care in the merely mechanical work; so that the cost of a "one-quarter" of 170° is nearly or quite twice that of the same focus of 100°, of equal quality in all other respects.

Charles Stodder.

Reply to Mr. Stodder, "B," and Mr. Edwin Bicknell.

To the Editor of the 'Monthly Microscopical Journal.'

Washington, D.C., *November* 21, 1871.

Mr. Editor,—There is nothing, I think, in the argumentative parts of Mr. Stodder's letter of July 20th * which calls for any reply from me, but the paragraph on page 203 concluding "all these facts I presume Dr. W. will in due time communicate to the public," demands a few words.

In January last I ordered, through Mr. Stodder, a high-power immersion objective of Mr. Tolles for the Army Medical Museum, "to be constructed with special view to the study of lined test-objects," leaving details of precise power, angle, price, &c., entirely to the maker, and stating to Mr. Stodder that it was my desire "to have in the Museum an objective which you will be willing to have me exhibit as a selected specimen of Mr. Tolles's very best work."

In the latter part of June I had the pleasure of a visit from Mr. Tolles, who brought with him the new immersion $\frac{1}{5}$th referred to in such complimentary terms in my Note on the resolution of *Amphipleura pellucida*.† Mr. Tolles had with him at the same time two immersion $\frac{1}{10}$ths and an immersion $\frac{1}{15}$th, the performance of which was exceedingly creditable; but as it was essentially similar to that of other objectives of his previously referred to by me in this Journal, I limited my published remarks to the one of which I could conscientiously speak as superior in definition to any similar power which I have examined.

At the same time I engaged Mr. Tolles to undertake the construction of a higher power objective for the Museum, which, however, has not yet come to hand. Should it fulfil Mr. Tolles's expectations it will give me as much sincere pleasure as it can to him or any of his friends, and I will promptly make the facts public.

My September paper also contained my determinations of the true

* This Journal, October, 1871, p. 201. † Ibid, Sept., 1871, p. 150.

magnifying power of my Powell and Lealand's $\frac{1}{16}$th when used with its immersion front, so that it now only remains to say a word on the difference which Mr. Stodder refers to between his Nobert's plate and mine. Mr. Tolles had Mr. Stodder's plate with him during his visit; it is mounted on a thick bottom glass, and is a facsimile of one belonging to Dr. Barnard, which served for my early work on the plate. I was therefore perfectly familiar with its characters. On the other hand, the plate now used in the Museum is mounted on a very thin bottom glass, permitting therefore the use of a condenser of short focus and high angle. The chief points of difference between the two forms of plate are given in a printed 'Description of the Nineteen Band Test-plate of Nobert,' issued nearly two years ago from the Museum, with a series of photographs of the lines, and a copy of which is in Mr. Stodder's possession. I am at a loss therefore to understand his remarks in this connection, but may add to what I have heretofore written, that on thick-bottomed plates, like Mr. Stodder's, the spurious lines (counting from thirty to forty in the nineteenth band, for example) are even more deceptively like the true lines than on thin-bottomed plates, and that a count is even more indispensable in the first instance than in the second.

I must also ask of you space for a few words with regard to two communications in your November number. On page 241 a correspondent, who signs himself B., and who seems to write very fairly, asks of me the aperture of the Tolles's $\frac{1}{5}$th mentioned in my Note on *Amphipleura*. I take pleasure in giving the information desired. The lens in question has two fronts—one of moderate angle for use dry. I make its aperture 90° at uncovered, 110° when corrected for cover by one full turn. The other front may be used either dry or wet. When the posterior combination is separated from the front as far as the screw collar will move it, the objective is fitted for the study of dry uncovered objects, and has an angle of 110°. One full turn of the screw collar corrects the objective for the thickest cover through which it will work dry. Its angle is then 140°. Without further change of adjustment a drop of water may be introduced, and it is fitted for the study of uncovered objects wet; another turn and a half may then be given, when it is corrected for the thickest cover through which it will work wet, and has an angle of 170° or upwards. The price paid Mr. Stodder for this objective, including both fronts, was one hundred dollars (United States' currency).

In your November number, also, on page 225, is a paper by Mr. Edwin Bicknell, of Massachusetts, who wilfully misinterprets my published measurements of the wet front of Powell and Lealand's $\frac{1}{16}$th, and thinks proper to use the following words (p. 226):—"I call it nothing more nor less than *deception* in Powell and Lealand (or any other makers) in marking (SIC) an objective nearly 33 per cent. less than its actual power, thus misleading people who cannot make actual comparisons; and I consider Dr. Woodward guilty of an equal amount of deception in *knowingly* putting forth work done by that objective as having been done by a $\frac{1}{16}$th." This language is *vulgar*, *insolent*, and *offensive*, and I should take no further notice of the paper in which it occurs if it did not seem to me that fair play to Messrs.

Powell and Lealand required a full statement of the facts with regard to such of their objectives as I have measured.

The Surgeon-General's Office possesses a number of objectives by the leading American, English, and Continental makers. Among these are the following by Messrs. Powell and Lealand. One $\frac{1}{50}$th and one $\frac{1}{25}$th, both dry lenses. Two $\frac{1}{8}$ths and two $\frac{1}{16}$ths, each furnished with two fronts, one for use wet and one dry. One of the $\frac{1}{16}$ths was originally made for dry work only, and the wet front subsequently added by the makers; the other $\frac{1}{16}$th and the two $\frac{1}{8}$ths were furnished in the first instance with two fronts. The following table gives the magnifying power of each of these objectives without eye-piece at four feet (48 English inches) distance from micrometer screen. The micrometer used was a broken Nobert's plate mounted with the lines uppermost, and the image of the micrometer was projected on the screen by direct sunlight.

Objective.	Magnifying power when uncovered at 48 inches.	Magnifying power at full correction for thickest cover same distance.
Powell and Lealand's $\frac{1}{50}$th dry	2200	3100
" " $\frac{1}{25}$th "	1250	1425
" " dry $\frac{1}{16}$th (No. 1)	790	930
New immersion front to ditto	975	1180
Powell and Lealand's immersion $\frac{1}{16}$th (No. 2) ..	900	1100
Dry front to ditto	770	910
Powell and Lealand's immersion $\frac{1}{8}$th (No. 1) ..	450	500
Dry front to ditto	425	490
Powell and Lealand's immersion $\frac{1}{8}$th (No. 2) ..	460	505
Dry front to ditto	450	510

The above determinations were made with the utmost care, each observation being several times repeated. No eye-pieces or amplifiers were used in order that the conditions might be as simple as possible, and a long distance was taken, that small unavoidable errors in its measurement might have as little influence as possible on the results. The magnifying powers were obtained by dividing the dimensions of the part of the micrometer selected into the dimensions of the image, carefully measured on the screen, and the results therefore fairly represent the performance of the objectives named *under the conditions stated.*

How are these results to be interpreted, and how ought the objectives in question to be named? Mr. Charles Stodder tells us* that "objectives are named when adjusted for uncovered objects, a fact not generally known by purchasers. The power increases, *i.e.* the focus is shorter as the collar is turned to work through the covering glass." But how in the case of a single objective with wet and dry front, or one which by mere change of the screw-collar works from dry to wet? I believe that the commercial practice in this case is to name the objective by its performance at uncovered dry, and this practice

* This Journal, October, 1871, p. 203, *note.*

may be justified by its convenience, as enabling a single trade-name to be given to the combination. Scientific accuracy would seem to demand that the maker should at least state the limits for each combination, but I know of no maker anywhere who has as yet adopted this plan.

Different modes of computing the focal lengths of objectives from their magnifying power under given conditions have been proposed. I have been in the habit of using a very simple rule, *viz.*:—The equivalent focal length of the objective in fractions of an English inch is of course equal to the distance from the optical centre of the objective to the screen, divided by the magnifying power. Since the optical centre cannot be determined with precision in compound objectives, we may substitute, without material inaccuracy, the distance from the micrometer to the screen, provided this be made so great that the unmeasured quantity represents a very small fraction of it. For all objectives from the $\frac{1}{5}$th upwards, therefore, if the distance from micrometer to screen be considerable (say such a distance as I have taken above), the magnifying power at that distance divided into the distance will give the focal length with reasonable precision.

For longer focal lengths, or in any case those who demand the nearest approach to accuracy may use Mr. Cross's formula,* $f = \frac{n\,l}{(n+1)^2}$ where n is the reciprocal of the magnifying power, and l the sum of the conjugate foci or the distance from micrometer to screen. Taking then the magnifying power at dry uncovered as the basis of the calculation, the above lenses of Powell and Lealand may be characterized as follows in decimals of an English inch.

Name of Objective.	Value by the Simple Method.	Value by Mr. Cross's Method.
Powell and Lealand's $\frac{1}{50}$th	·0218	·0218
" " $\frac{1}{25}$th	·0384	·0383
" " $\frac{1}{16}$th (No. 1)	·0607	·0606
" " $\frac{1}{16}$th (No. 2)	·0623	·0622
" " $\frac{1}{8}$th (No. 1)	·1129	·1124
" " $\frac{1}{8}$th (No. 2)	·1066	·1062

It will be perceived that all the above objectives therefore closely approximate, when worked dry uncovered, to what their nomenclature would demand, the $\frac{1}{8}$ths deviating most from the standard, and being in fact, more nearly $\frac{1}{9}$ths than $\frac{1}{8}$ths. It must also be borne in mind that with the most sincere endeavours on the part of the maker, it is not easy to give precisely the same magnifying power to any two objectives, and if any commercial titles are adopted a certain amount of deviation from the standard indicated must be expected.

With regard to the QUALITY of the performance of the above

* 'On the Focal Length of Microscopic Objectives.' By C. R. Cross. Boston, 1870.

lenses I may add the following remarks. Worked with their immersion fronts, I regard the $\frac{1}{8}$ths and $\frac{1}{16}$ths above mentioned as superior in defining power to the dry $\frac{1}{25}$th and $\frac{1}{50}$th of the same makers. This is shown on test-objects, such as the *Amphipleura pellucida*, &c., and on anatomical preparations, as well as on the Nobert's plate. The immersion objectives in question combine, with exquisite defining power, *unrivelled flatness of field* and *depth of penetration.* In all these qualities the two immersion $\frac{1}{16}$ths excel any objectives it has been my fortune to obtain from any source. I make this statement with pleasure, out of justice to the makers, with whom I have no personal acquaintance. I should take still greater pleasure in being able to announce that I had obtained from them, or from any other makers, still better objectives.

Very respectfully,

J. J. Woodward, U. S. Army.

Correction of Dr. Boyd Moss' Paper.

To the Editor of the 'Monthly Microscopical Journal.'

Ceylon, *Nov.* 15, 1871.

Dear Sir,—Kindly supply in your next No. the following corrections to my paper in that for October 1st.

For the "cilia are raised on a substructure of a *cave*-like form," read "*wave*-like."

I was in error, too, in saying that the corpuscles of the blood agreed in size with the musk deer of India, as these latter are only about $\frac{1}{12500}$ diameter, the same as those of the small so-called "moose deer" of Ceylon.

Boyd Moss.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *December* 6, 1871.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, and a vote of thanks passed to the respective donors.

The Secretary called the attention of the Fellows to a small lamp devised by Mr. Richards, which could be easily carried in the pocket; and also to a portable stand to which the lamp could be fitted. The light given by this lamp was sufficient for all ordinary purposes of

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

the microscopist, and the flame though small was sufficient, with the help of the bull's-eye condenser, for the illumination of most opaque objects. The price with stand was, for the paraffin lamp, 10*s.*; for the spirit lamp, a modification suited to benzoline, 7*s.* 6*d.* The thanks of the meeting were presented to Mr. Richards for exhibiting this lamp.

The Secretary announced that a communication had been received from Dr. Anthony, of Birmingham, relating to the "Markings of the Battledore Scales of the Lepidoptera;" and also from the Chevalier Huyttens de Cerbecq in reference to the minute structure of the scales of insects, and especially bearing upon Mr. McIntire's observations upon this question. The Chevalier's note was accompanied by a present of slides of selected scales.

Mr. J. Beck suggested that when papers were "taken as read" through press of time, an opportunity should be afforded the Fellows of discussing at the following meeting any points of interest that might be found in such papers.

Dr. Beale then read a paper "On the Nerves of the Capillary Vessels and their Probable Action in Health and Disease."

The President proposed, and the meeting unanimously accorded, a vote of thanks to Dr. Beale for the paper which he had read.

Dr. Berkart said he perfectly agreed with Professor Beale on his throwing some doubt on the supposed influence of the nervous system on nutrition. There was a tendency at present to refer almost all pathological changes primarily to the nervous system, without there being sufficient anatomical facts to prove the assumption. There was, for instance, a class of diseases called *Tropho-neuroses.* The substance of the muscular tissue was entirely disregarded, and the atrophy observed in the muscles traced to disease of the nerves. Now he had lately seen a case of atrophy of the right pectoral muscles in a sailor who had been in the habit of pressing the rudder always against the right side of his chest. After death he (Dr. B.) could not find any trace of disease in any part of the nervous system, though the whole muscular tissue was replaced by connective tissue. It was therefore, and especially in this case, more natural to suppose, that the pressure that primarily acted on the muscle itself (the *myoline*) had destroyed it, without the active intervention of the *Trophic nerve.* Though in very many cases of nutritive changes the direct influence of the nervous system could not be made out, there were, however, many physiological experiments which forcibly showed the influence of the nerves on secretion. He would remind the Fellows of the experiments of Claude Bernard, Ludwig, Eckard, and Adrian on the salivary glands. The difference in quantity and quality of the saliva obtained in the various experiments could not well be explained, without allowing the nervous system some share in the cause of it.

Dr. Murie said Dr. Beale had very clearly divided the contents of his paper into two headings; under the first head treating of facts, under the second of theories. With regard to facts, Dr. Beale's name was a sufficient guarantee that what he said might be taken on trust. As respects his theories, more liberty of judgment might be

exercised. Dr. Beale's facts appeared to him very feasible in many points. His reasons for this assertion he would state. Many present were doubtless aware that the capillaries of the larger-sized vessels which formed the *rete mirabile* of the porpoise were of a very remarkable character. In them we had appearances similar to those described in some of Dr. Beale's diagrams, *viz.* contractile vessels of moderate calibre, and minute vessels supplying these; the capillaries sometimes running parallel, sometimes crossing each other at various angles. It was very difficult to comprehend and explain what was the cause of the steady and uniform circulation in these masses of blood-vessels considered in the light of the older ideas on the question. Amongst these capillaries we had also nerves to a certain extent freely intermingled: and when tracing them by ordinary scalpel manipulation, similar ramifications to what had been demonstrated by Dr. Beale in his minute dissections were apparent, wherefore we might look for some nervous influence as the cause of the blood's propulsion by the contraction of the multitudinous vessels of the *rete*. The doctrines nowadays were different to those which obtained in former times, in consequence of the closer study which had been bestowed upon microscopic anatomy. Each organ was found to be a distinct piece of machinery. All present well knew that if the connecting cord of an electrical battery be cut, motion at once ceased at the farther end, and what Dr. Beale had shown reminded him (Dr. Murie) of what obtains and could be readily seen in the body of the torpedo. But before alluding further to the electric organ of that remarkable fish, he would ask Dr. Beale whether, in speaking of the section of the mole's nose, it was a vertical aspect to which he referred? (Dr. Beale explained that he referred to the end of the nose looking down upon it.) That fact then convinced him (Dr. Murie) that the same thing as Dr. Beale described occurred in the nerves and electric battery of the torpedo. In the organ in question there were to be seen a series of hexagonal areas or vertical columns, containing jelly-like or protoplasmic substance precisely similar to what was shown in one of Dr. Beale's smaller diagrams. These in the fish's battery could be discerned with the naked eye; and moreover, when using the microscope, a multitude of delicate nervous twigs was revealed, which ramified between and abutted against the sides of the hexagonal battery cells. Max Schultze and he believed Kölliker among others traced these terminal nerves in the same position as Dr. Beale had shown in the mole's nose. He (Dr. Murie) wished to point out that if there was in the case of the torpedo a vast electrical battery supplied by nervous influence of gigantic power, was it not very probable that the same kind of thing obtained in the arterial capillaries, modified of course to the limited exigencies of their contractile powers? Having regard to the existence of the mole's nose, he (Dr. Murie) was disposed to agree with Dr. Beale in the demonstration he had given of the subject in general. The theoretical portion of Dr. Beale's paper was not altogether "plane sailing." He believed, however, that the doctrines enunciated did explain very many things both of a pathological as well as physiological character.

He was inclined to agree with Dr. Beale, that nerves did not enter those epithelial tissues when the epithelium is thrown off continuously; for in such a case the nerves would be unfavourably exposed, and undoubtedly placed in a dangerous position, to retain their normal stimulus. Dr. Murie, in conclusion, eulogized the paper just read as one of those valuable contributions to microscopic anatomy unfortunately too seldom laid before the Society, where, according to the number of its members, such practical researches ought to be the rule and not the exception.

Mr. Stewart said he wished to mention, in reference to the question of the existence of nerves amongst epithelium cells, that having recently had the opportunity of examining some very beautiful specimens of the cornea, prepared by Dr. Klein, he was firmly convinced of the existence of a fine plexus of nerves between the cells of the conjunctival epithelium, directly continuous with the coarser plexus of nerves situated in the middle layer of the cornea.

With regard to the influence of nerves on secretion, he thought that the altered character of a secretion when the nerve supplying the gland was severed or irritated, could not be entirely explained by the influence it would have upon the blood-vessels. There could be little doubt that there was a direct influence upon the cells, but the question was, how that influence was exerted. Admitting the close analogy between nerve force and electricity, to a certain extent some of the phenomena observed might be explained by the example of a capillary tube, which retained water under ordinary circumstances, but if, when full, an electric current be passed through it, the water immediately flowed out: in dialyzing membranes also, the character and quantity of the fluid which passed was modified if an electric current was passed through the membranes.

Dr. Beale asked whether the section Mr. Stewart had seen was transverse or not.

Mr. Stewart said it was not. The fibres seen could be traced down through the various forms of plexus most distinctly.

Dr. Beale: What was the specimen mounted in?

Mr. Stewart: Glycerine.

Dr. Beale: I should like very much to see a specimen in glycerine; I have never been able to satisfactorily develop the nerves in the conjunctival epithelium.

Mr. Stewart: The surface of the epithelium could be distinctly discerned, and from thence the fine nerves could be traced down until they joined the nerve situated in the middle layer of the cornea.

The President asked Dr. Beale whether he meant to deny altogether the contractility of the capillary vessels.

Dr. Beale said the exception occurred in the case of young creatures, such for instance as the tadpole, whose tail was contractile.

Dr. Lawson said he thought the gratitude of the Fellows was due to Dr. Beale for the paper he had read. He considered it one of the most valuable papers which had been brought forward on the subject for the past five or six years. He thought that their views of microscopical anatomy had been very much advanced by it. So far as he

could judge, the works written by foreigners had not gone into the matter so far as Dr. Beale had gone in observations upon the nerves. Their glasses had not the power which Dr. Beale had brought to bear upon the subject. He was for this reason disposed to give a distinctive prominence to Dr. Beale's researches. He had advanced considerably beyond what had been done on the Continent, and especially in regard to the distribution of nerves upon the blood-vessels. He would just ask, in reference to the small particles described as projecting from the edge of the capillaries, whether those small particles might be converted into blood corpuscles. On another point, he would mention in regard to the capillaries in the frog's foot, that he quite agreed with what Dr. Beale had said. A great number of experiments had been made in regard to the effect certain substances produced on the capillary circulation of the frog, and he (Dr. Lawson) had gone over them himself invariably with the same results as Dr. Beale had shown; *viz.*: that the effect on the blood-vessels was due entirely to the action of the nerves, and not to the influence of the substance employed in the experiment.

Mr. Hogg said perhaps it might not be known to every member of the Society just how the question in debate stood. The subject, although a moot one, was a very interesting one. For years it had been a matter which greatly puzzled physiologists, to know how the blood was propelled from the capillaries back again into the circulation. It had been attributed in a great measure to the action of the heart, but it was very well known that amongst the *invertebrata* many have no heart at all. There we had creatures that propelled their blood from some cause that we could not explain. There was also in vegetable forms a circulatory movement that went on continually without any apparent nervous structure, or without any centre of motion whatever. It was reasonable to suppose that the circulation in minute capillary vessels was carried on in a similar manner. It was known that the rate at which the blood moved through the capillaries was about 2 inches per minute. The question as to how this circulation was carried on was a very important one, having special bearings upon pathological subjects, such, for instance, as inflammatory action. He thought Dr. Beale had done good service in treating as he had done on the nerves of the capillaries, because so far microscopists had not been able to discover any contractile power in the capillaries at the adult stage of existence. He wanted much to know how the blood was squeezed out of the capillaries and carried back into the veins again. He had undertaken some few years ago a series of investigations, on behalf of the late Mr. Guthrie, into the nature of those capillaries, Mr. Guthrie being at the time much interested in the structure of non-striated muscular fibre, and he (Mr. Hogg) well remembered the fact of having passed over the nerves in connection with their structure. He had attributed the swellings observed to swellings in the non-striated muscular fibre, entirely ignoring the presence of nerves. Dr. Beale had mentioned that he believed that the nerves acted upon the muscular fibres rather than upon the walls of the capillaries. Perhaps that was so. He, however, should think there was direct action upon

the capillary vessels. Then with reference to the cornea, where we had a beautiful example of nervous structure; nerves existed there probably for the purpose of preventing the entrance of blood into its structure. Since it had not been demonstrated that there was contractile power in capillary vessels, it was reasonable to suppose that the nerves acted in the manner indicated by Dr. Beale.

Dr. Leared asked whether Dr. Beale had extended his observations to the minute vessels of the brain, and whether he had been able to make any deductions in regard to the ordinary but not satisfactorily explained phenomenon of sleep. He thought Dr. Beale's views would throw great light upon the question of sleep, and also as to the action of certain drugs, such as *bromide of potassium* (which probably consisted in their power of controlling the cerebral circulation), found so effectual in restraining epilepsy and allied affections.

Dr. Beale, in answer to Dr. Leared, said he had not been able to demonstrate conclusively the existence of nervous fibres in connection with the capillary vessels in the brain, up to the present time. He thought the turtle's brain would be the best one to study with reference to this question.

Dr. Beale then said, with regard to the remark of Mr. Hogg as to the direct action of the nerves on the capillaries, that in his opinion it was doubtful if nerves acted directly upon capillaries, or upon the elementary cells of secreting glands. He should explain the action as reflex. It would be difficult to account for direct influences until some theory had been propounded to explain the way in which the nerves might be supposed to act upon cells and non-contractile tissues. He did not see how we could admit the doctrine.

The remarks made by Dr. Berkart concerning Eckard's observations on the salivary glands were to the point, but he thought that, after all, change might be due to the influence exerted by the nerve fibres in the small arteries. He did not think any explanation yet offered wholly satisfactory.

Dr. Murie had in the course of his remarks advanced the opinion that many nerve organs were like electrical apparatus, and in this opinion he fully concurred. He (Dr. B.) differed from many of those who brought forward hypotheses of nervous action, and he was quite prepared to admit that nervous action generally might be really electrical action. Admitting this, however, it must be borne in mind, that there was a great difference between the electrical machine and the nerve machine of the organism. The first was made by man, the other made itself, or was evolved as the saying was. He thought Dr. Murie quite correct in the argument he had adduced with regard to the structure of nerve organs being like the battery of the torpedo. Many other animals possessed forms of apparatus which undoubtedly did produce electricity in the organism, and it was only reasonable to infer that other forms of apparatus exhibiting the same type of structure produced electricity in smaller amount. The way in which the circulation was carried on was very much the same in the highest as in the lowest animal; and so it was probable that with regard to the distribution of the nerves a general agreement in fundamental

character would be found to obtain. All facts bearing upon this general question were undoubtedly of the greatest interest, and too much time could not be devoted to their elucidation. He did not feel disposed to agree with the views now in favour and supported by Mr. Stewart with reference to the termination of the nerves in the epithelium. He was ready to be convinced on seeing specimens, but those he had examined were not satisfactory, nor had those who entertained these views shown how the nerves reached the ultimate points to which they traced them. Did the nerves get into the epithelium when it was young, or wait till the epithelium was formed? In discussing the subject a great number of difficulties would present themselves in the way of giving a feasible account of the distribution of nervous fibres in the epithelial structure. He thought many of the drawings now made in Germany open to very severe criticism. For instance, there was a paper on this question by Schöbl, considered by many who had seen it a very elaborate and beautiful memoir on the distribution of the nerves of the bat's wing. He thought that Schöbl must have drawn upon his imagination, or had trusted the designing as well as the execution of the beautiful lithographs accompanying his paper to an artist ignorant of structure. He (Dr. Beale) had worked at the subject for a long time, and he would defy anyone to prove that the nerve fibres in the trunks of the nerves ran in a direction parallel to one another, as represented in all Schöbl's drawings. He challenged anyone to show him the smallest portion of nerve fibre in which the nerves lie parallel to one another, as represented in the drawing, say for a distance of only the fiftieth part of an inch. No one who had carefully examined specimens in which the nerves were clearly demonstrated would have permitted them to be delineated in the way represented. Everyone who had dissected nerves knew that they did not lie one beside the other; for if that were so, nerves would tear longitudinally. He thought that those who undertook the office of critics should examine the published representations of different anatomical observers in this country, as well as abroad, and carefully compare the various conclusions before they committed themselves to a doctrine which within six months might be supplanted by another of which they would speak with equal confidence and equal favour. They should see the specimens which have been prepared by observers who have worked at the subject, and in different ways; and when possible, the critics should make themselves acquainted with the points at issue, and endeavour to represent, without favour, the views of different observers, instead of, as is too often the case, siding with one, and endeavouring to carry the opinion of others one particular way. In England the views of foreigners had, he thought, a very undue advantage; they were accepted and excessively praised, while many English critics scarcely noticed the conclusions of English observers. He alluded to the subject because the general indifference shown in certain quarters to the investigations of students in this country discouraged the study of minute anatomy here. He would be happy to show his specimens to anyone who would be willing to devote an hour or two to their investigation.

The meeting was then adjourned to the 3rd January, when W. Carruthers, Esq., F.R.S., will read a paper.

Donations to the Library and Cabinet, from Nov. 1st to Dec. 6th, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Spot Lens for Low Powers	*Mr. Chas. Baker.*
Journal of the London Institution, No. 8	*Institution.*
Quarterly Journal of the Geological Society, No. 108	*Society.*
Two Slides of Micro-ruling on Glass. By John S. Stanistreet, Esq.	*Author.*
Twelve Slides of Insects' Scales	*Chevalier H. de Cerbecq.*

The following gentlemen were elected Fellows of the Society:—

Joseph F. Payne, B.A. Oxon.
John Rogers, Esq.
Charles H. Roper, M.R.C.S.
Charles Croydon, Esq.

Walter W. Reeves,
Assist.-Secretary.

Brighton and Sussex Natural History Society.

October 12th.—Ordinary Meeting. Mr. W. M. Hollis, President, in the chair.

Messrs. E. H. Moore, A. Creak, J. Cobbett, and Helmsley, were elected ordinary members.

Mr. Wonfor announced the receipt of 'The Cruise of the Norna,' by Mr. Marshall Hall, from the author, to whom a vote of thanks was given.

Mr. C. P. Smith announced the discovery of an umbelliferous plant, new to Britain, near the Race Hill. It was not only a new plant, but a new genus and species as well.

A paper by Dr. Stevens, of St. Mary Bourne, "On the Flint Works at Cissbury," in the absence of that gentleman, was read by Mr. Wonfor.

After alluding to the peculiar botany and zoology of the South Downs, and pointing to the evident connection existing between some of the highest peaks, both in a geological point of view and also as positions likely to be occupied, either as camps of defence at one time, or as a series of encampments to keep down a subjugated race, at another, especial attention was directed to Cissbury, as bearing marks not only of having been occupied as an entrenched camp during Roman days, as evidenced by implements, pottery, &c., found in and about the sixty acres enclosed within the ramparts or entrenchments, but to its having been what might be called the "*Flint* Sheffield of Sussex," where an early people fashioned rude flint implements, and where they or a later people, used a very rude kind

of pottery. Colonel Lane Fox found about 600 chipped implements of different kinds, which he considered might be referred to the *neolithic* period. Upon a recent visit, in company with Mr. J. P. M. Smith and Mr. Wonfor, he had taken away 100 wrought flints, many of rude type, but all exhibiting manifest marks of design. Some were well shaped, especially some lance heads, while flakes, picks, slingstones, and edged tools, abounded in the circular pits, and on the surface of the encampments. Nowhere had he found the rubbed implements, which are not rare in Hampshire. The implements recently thrown out of the pits showed fresh surface patination, while those scattered about the hill, having undergone long atmospheric action, showed a change of colour, to the depth, in some cases, of a quarter of an inch. The question how the occupants of the hills could obtain water, their possible hunting grounds, together with the cattle probably kept by them, were next discussed, and reference was made to a Roman building and a subterranean trench and caves beneath it, occupied by a flint-using people, lately excavated by him at Finkley, Hants.

October 26th.—Microscopical Meeting. Mr. W. M. Hollis, President, in the chair.

The subject of the evening, "The Scales of Insects," was introduced by Mr. Wonfor.

By the term "the scales of insects," was understood those epidermal appendages found on the head, thorax, legs, and abdomen of some insects; on the wing cases or *elytra* of some beetles; and on both surfaces of the wings of butterflies and moths; to which they gave that wonderful beauty of colour which rendered them objects of admiration even to the non-entomologist.

In general terms, scales might be designated as flattened hairs; though, in examining the multitudinous forms and varieties found among insects, every stage between the round hair and the purely flattened scale might be found. The analogy to hairs was also seen in their horny character.

Taken as hairs, they seemed to present three different types: one very common one being those in which there was an upper and lower surface, more or less rugose, striated or wrinkled, with an inner structureless membrane between them, which seemed to act as a kind of foil to throw up the brilliant colour of the scales. In another type, the upper and under surfaces, without the inner structureless membrane, were welded together; for, where the scales were broken or damaged, no trace of the intermediate membrane could be detected. In a third type, the scales were more or less round, sometimes tasselled, and devoid of the inner membrane. In these last, and possibly also in the first type, there seemed to be a power on the part of the insect of inflating or puffing out the scales, so as, possibly, to render them more buoyant. Certainly these scales, when taken from fresh-caught specimens, were rounded, but, when pressed between glass, they became flattened.

Each scale was inserted in the wing membrane by a stalk, and here, especially among the lepidoptera, the creatures appeared able not

only to inflate, but also to raise the rows of scales. Some of the "tasselled" scales, beside their points of insertion, had also a kind of ball-and-socket movement. When scales were taken from recently-caught specimens, a slight pressure of the covering glass caused an oily substance to ooze from the point of insertion; whether this was pigmentary matter or a circulating fluid, was doubtful.

Considerable difference of opinion prevailed as to the markings on scales, and the learned were at issue on this point. It would seem there were several types. In some, longitudinal striæ or ridges were crossed at intervals by transverse striæ; in others there were longitudinal ridges with puckerings or wavings of the membrane between them. Some had urged that the longitudinal striæ were of a beaded character, and in some scales the lower beads could be seen through the structureless membrane. He had found that, when either the upper or under surface was torn or damaged, the so-called under beads could not be seen, leading to the idea that they were only spectral reflexions of what had been termed surface beading. While it was difficult to disprove the beading and to demonstrate the continuous ridges in the Thysanuridæ, they might be well seen in some of the large and well-marked scales from the larger foreign and English butterflies and moths. Some markings were evidently due to pigment, which in many cases presented quite a granular appearance.

He was prepared to exhibit and illustrate what he had advanced by a large collection of scales, *in situ* and separated, and some of the larger ones broken up to show structure; and by the kindness of some London microscopical friends, Messrs. T. Curties, McIntire, and Marshall, he was also able to show some very choice test-scales from Thysanuridæ, Poduridæ, &c.

The meeting then became a conversazione, at which a number of very beautiful objects in illustration of the paper were exhibited by Dr. Hallifax, Messrs. Glaisyer, R. Glaisyer, Sewell, C. P. Smith, and Wonfor.

Mr. C. P. Smith exhibited a very compact, cheap, and handy portable microscope, by C. Baker, of Holborn. This instrument, which was much admired, possessed a revolving stage so contrived that the object under examination always remained in view during revolution. This was an important adjunct, and hitherto had only been applied to the more expensive form.

Mr. J. Robertson exhibited a wing of *Sirex gigas*, showing the spines.

South London Microscopical and Natural History Club.

An Ordinary Meeting of this Club was held on Tuesday, October 17th, at Glo'ster Hall, Glo'ster Place, Brixton Road. Mr. Deane, F.L.S., presided.

Dr. W. M. Ord read a paper "On Polyzoa," of which the following is an abstract:—

Polyzoa are microscopic animals that one finds tolerably abundantly both in fresh-water ponds well shaded by trees, and on all sorts

of fixed structures at the sea-side. The fresh-water polyzoa, which you will encounter readily about here, will be perhaps those most familiar to you, and so I propose to introduce one of these fresh-water kinds as a type of the rest. All this great fungus-looking mass is composed of a number of animals congregated together; this was captured in Mr. Thrale's lake on Tooting Common. If now you look at the hard structure, you will see that it presents at all points little cavities, which are the openings of long tubular cells into which the softer substances are retracted. These cells are lined by delicate animal matter; and this matter projects beyond the mouth of the cells in the form of a soft tube of a somewhat conical shape. The cone, at its termination, suddenly expands into a structure called the *lophophore*, or plume-bearing structure. In most of the familiar forms, this is in the shape of a horse-shoe placed flat, and along the sides of this horse-shoe run two series of tentacles parallel to one another. These expand into a beautiful crown, which may be compared to the body of one of the old-fashioned chariots. On the disk where the lophophore is attached, we find the commencement of the alimentary canal—the mouth—enclosed by the arms of the lophophore, and lying between the double row of the tentacles. On viewing the living animal you will see that these tentacles by means of their cilia cause little whirling currents, so arranged as to bring solid matters suspended in the water down into the hollow cup, and thence into the mouth; from the mouth they go to the *pharynx*, which is richly fringed with cilia, and thence to the stomach. In the walls of the stomach we find usually a number of little cells, containing dark-brown matter, which are supposed to discharge the function of a liver. Surrounding the alimentary canal is a cavity called the *perivisceral* cavity. This contains a fluid, which is all the creature has to represent blood; this fluid circulates in all parts of the body. As to their respiration, they draw oxygen from the water around them; this is done in part by their tentacles, also by drawing in water, with regular alternating actions of suction and ejection. With regard to the organs of reproduction, one may note that these creatures reproduce themselves in at least three ways. First, by eggs, the result of the impregnation of ova with spermatozoa; the ova undergo their development within the body, as far as can be made out. The embryo is first a hollow sphere; a layer is then thrown off from the surface at the same time that an opening is made in the wall of the sphere; a second sort of little sphere is thus formed within the first, and here little polypides are gradually developed. A second way of reproduction is that on the side wall at some point or other a projection occurs, and grows to a tube in which all the parts of a new polypide are by degrees developed; and by a third form of development, we find towards the latter part of the autumn dark, flat, oval bodies forming themselves. They are composed of two slightly convex plates like watch-glasses placed face to face, with germinal matter inside. These eggs live through the winter from the autumn, nearly all the composite structures dying as winter comes on. These bodies receive the name of *statoblasts*, from the idea that they remain in the dead organism silently awaiting the spring. Their forms

are very varying and beautiful, and some of them are most lovely microscopic objects.

A word now as to the systematic position of the polyzoa. The more we know of their homologies, the more reason there is to class them with the lower class of mollusks—those called the *brachiopoda.* It is suggested that we can join the brachiopods and polyzoa, just as among the *tunicata* we find isolated forms as clearly as possible connected with forms as social and interdependent as are the polyzoa. I will now say a word, for the benefit of those who know perhaps little about the subject, as to the zoological position of these polyzoa. They seem first to have been noticed by the celebrated Abbé Trembley, the same who taught us about the little fresh-water hydra; how it may be cut in pieces and otherwise mutilated without any apparent harm, and how each fragment grows for itself the parts which have been removed. In investigating these undoubted polyps, he discovered some forms which he called the *polypes à panache*, or plumed polyps. Some years after, Ellis took them up, and in his system they were still associated with the ordinary polyps. In 1828, Milne Edwards, the French zoologist, first pointed out their distinct homologies with the tunicata, and since that time they have been put at the lowest part of the sub-kingdom mollusca. But while general consent classes them with the mollusca, some reasons seem to exist for classing them with the worms; Dr. Strethill Wright having discovered a fixed marine *annelid*, which had a kind of horse-shoe plume at its free extremity; still, when we take the great resemblance of the adult polyzoa to the structures of the brachiopoda and tunicata, we may be satisfied in our minds of the wisdom of placing this class among the mollusks, at the bottom of the list of the classes included in that sub-kingdom.

Dr. Braithwaite observed that many of the specimens exhibited had the tentacles expanded. He would be glad to know how this was managed.

Mr. Stewart said he had succeeded in keeping them out by adding a few drops of the best French brandy to the water in which they were living. They seemed to appreciate this beverage so highly that they were overcome by the liquor, and died with the plumes expanded. They could then be mounted in the ordinary way.

A vote of thanks was unanimously accorded to Dr. Ord for his interesting paper. The President announced the first soirée of the club, to be held at the Horns Assembly Rooms, Kennington, on Thursday evening, November 30th; also a paper for the next meeting, on Tuesday, November 21st, by Mr. Jackson, "On the Barks of Trees."

An Ordinary Meeting of this Club was held on Tuesday, November 21st, at Glo'ster Hall, Glo'ster Place, Brixton Road. Mr. Deane, F.L.S., presided.

Mr. Jackson, of the Kew Museum, read a paper "On the Barks of Trees," of which the following is an abstract:—

I propose to-night to say a few words upon the variable structures and characters of a few of the most remarkable barks of foreign trees. I would remind you that in *exogenous* structure, to the con-

sideration of which I shall confine myself, there are three distinct series or layers; first, the inner bark or *liber*, called the *endophlœum*, which is composed of long, fibre-like cells; secondly, the cellular portion or green bark, called the *mesophlœum;* and thirdly, the corky envelope or *epiphlœum*, which is sometimes very thick, as for example the cork of commerce, which is certainly a rather unusual development of the outer layer. From the inner barks are derived most of the fibres used for making into cordage, matting, or similar articles. One of these barks, the Lace Bark of Jamaica—*Lagetta Lintearia*,—is exceedingly beautiful and interesting, and it is moreover useful to the natives of the West Indies for many economic purposes. It is composed of a series of concentric layers of very fine and strong fibres, which, by crossing and interlacing each other, form a complete network, the beauties of which are quite hidden till the bark is beaten out, and the fibres partially separated by carefully pulling them in a lateral direction, when a piece of vegetable lace, a yard or more in width, will be produced. This natural lace is used in Jamaica for making caps, hats, collars, frills, &c., first being bleached by sprinkling with water and exposure to the sun. It is said that Charles II. was presented by the then Governor of Jamaica, with a pair of ruffles and other articles of dress made from this lace bark, and also that, in former times, the whips used for flogging slaves were mostly made from this bark. The bark of the Paper Mulberry of the South Sea Islands is another of the fibrous kinds; it is very strong and tough, and is used in the Pacific Islands for making what is called tapa cloth, which serves the natives for various articles of clothing. Another remarkable fibrous bark is the *Antiaris Saccidora*, called the Sack Tree in Western India and Ceylon. The bark of this tree is used for making sacks, hence its common name. A trunk is selected of the requisite diameter, and a piece is cut off, of the required length; the bark is then soaked and beaten, loosened from the wood, and turned back or inside out; if it is entirely stripped off, it requires simply to be sewn up at one end, but it is usual to leave a small piece of the wood to form the bottom. In the Natural Order *Myrtaceæ*, some very variable bark structures occur, for instance, in the Stringy Bark Tree of Tasmania—*Eucalyptus gigantea*,—which is toughly fibrous or stringy, while in the Iron Bark it is of such a compact solid nature, and so hard, that it might be taken for a close-grained wood, rather than a bark. Another very remarkable bark is that of the Pottery Tree of Para. It is the *Moquilea utilis* of botanists, and is a large straight-growing tree. A microscopical examination of the bark shows all the cells of the different layers to be more or less silicated. The name of Pottery Tree has been given to this plant in consequence of the uses to which the Indians apply the bark for making into a kind of earthenware. The bark is burnt, and its ashes mixed with clay, in proportions varied at the will of the operator. All sorts of culinary articles and cooking utensils are made from it; they are very durable, and will bear any amount of heat. Having now brought before your notice a few barks of very dissimilar structure, I will leave the matter in your hands to work

out more fully. I have mentioned only a few of the most interesting and peculiar barks that have come under my notice in the course of my duties at Kew; but I am persuaded that if microscopists would examine them more carefully, and devote even a portion of the attention to them that they have given to woody structure and to diatoms, some beautiful objects would be the result, and our knowledge of these matters would be considerably increased.

A vote of thanks was unanimously accorded to Mr. Jackson for his interesting paper.

Mr. Suffolk said, that although Mr. Jackson seemed to have hinted that the subject of barks would furnish little of microscopical interest to the members, he could assure them, from personal experience, that the study of the *liber* cells was most interesting. Shortly after the formation of the Quekett Club, he was appointed on a committee for the investigation of the fibres of flax and hemp, with a view to their discrimination in mixed fabrics. The committee did some little work, but the inquiries were ultimately abandoned, the fibres being so much alike that it was impossible to distinguish them under the microscope. He advised those persons who examined these fibres not to neglect the use of polarized light. In the hope of obtaining some further knowledge as to the structure of the fibre, he had immersed it in a solution of copper in ammonia, but found that the fibre was too rapidly destroyed. The use of nitric acid brought into view some secondary deposits, principally spirals; but, by simply placing the flax fibre under a thin glass with a little turpentine, and applying a power of 100 to 150 diameters, with polarized light, these structures can be plainly seen. Mr. Suffolk remarked that in his cabinets there were fifty specimens of fibres, supplied to him by the Indian Government, duplicates of which were to be found in the cabinet of the Royal Microscopical Society; and, in conclusion, he said that he brought forward this subject to show that it contained much of microscopical interest, and he could assure Mr. Jackson that his lecture was most welcome to the members.

Mr. James Love exhibited a new triangular prismatic aquarium, of his own invention; and explained, by means of a small model, the principles of its construction.

It was moved by Mr. Newman, seconded by Mr. Cottrell, and carried unanimously, "that the hour of meeting be in future eight o'clock, instead of half-past seven o'clock in the evening."

The President announced a paper for the next meeting, on Tuesday, December 19th, at eight o'clock in the evening, by Mr. J. Traill Taylor, "On the Photographic Delineation of Microscopical Objects."

Very fine nerve-fibres with connective tissue corpuscles and pigment cells, ramifying in the connective tissue at the base of the heart of the hyla. *a*. Nerve-fibre with nucleus. Its relation to the connective tissue corpuscles, *b*. and to the process of a pigment cell, *c*, is well seen. The fine nerve-fibre, less than the $\frac{1}{100000}$ of an inch in diameter, is not continuous with any of these structures. It passes near them, but is not incorporated with them. All are developed *pari passu*, but are structurally independent.
× 2800. 1862.

RELATION OF FINE NERVE-FIBRES TO PROCESSES OF PIGMENT CELLS OF THE FROG.

THE

MONTHLY MICROSCOPICAL JOURNAL.

FEBRUARY 1, 1872.

I.—*On the Relation of Nerves to Pigment and other Cells or Elementary Parts.* By Dr. Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.

(*Read before the* Royal Microscopical Society, *Dec.* 6, 1871.)

Plate VI.

The tendency of opinion in these days seems to be in favour of the conclusion that the finest branches of the nerve fibres come into structural relation with the active elements of other tissues. Many profess to have traced nerves into epithelial cells. A number of authorities agree in asserting that the ultimate nerve fibres come into actual contact with, and are probably in very close relation with the contractile tissue of striped muscle. Not a few assert that the nerve fibre may be traced into the nucleus of unstriped muscular tissue. Kühne concluded that the nerve fibrils were continuous with the prolongations of the connective-tissue corpuscles of the cornea, and M. G. Pouchet published a paper in the December number of the Journal with the object of convincing us that fine nerve fibres were continuous with—in fact ended in the pigment cells (chromoblasts) of the skin of fishes.

Upon the other hand, my observations have led me to the general inference that in no case do the finest terminal ramifications of the nerve fibres end in the manner described; and whatever may be the nature of the influence produced by the nerves upon the structure, and the action of various tissues and organs, I do not think that it is dependent upon continuity of substance between the nerve and the tissue affected.

From what I have seen I feel confident that at least in many cases the contraction of a muscular fibrilla depends upon a change in the nerve which runs *near to* it, but is distinctly separated from it—upon such a change as a varying intensity in the electrical current traversing the nerve fibre might occasion; and when contraction of protoplasm (bioplasm) follows upon irritation of nerve fibres, I believe the result is also due to the same circumstance, and

not to any direct influence propagated by the nerve to the protoplasm (bioplasm) by reason of continuity of material.

The drawing in Plate VI. will serve to illustrate the only conclusion I can accept at this time with reference to this interesting question. Although I publish only this one drawing, the fact has been observed in many instances, and in many different animals. In no case have I been able to satisfy myself that the nerve ends in the manner M. Pouchet describes, and which accords with the conclusions arrived at by some other observers who have studied somewhat similar structures. I believe, however, that upon this matter my own conclusions as regards the non-continuity of the nerve fibres with the cell are in accordance with the more recent observations of Dr. Klein, though this observer describes a plexus of excessively fine nerve fibres upon or over the protoplasm (bioplasm) of the corneal connective-tissue corpuscles, as demonstrated by the use of chloride of gold, but which is not seen in my specimens prepared according to a different method of investigation.

As far as I have yet been able to extend my inquiries I feel confidence concerning the results arrived at. I am quite certain of what I have described, and equally sure I can demonstrate the facts to others. That there may be still finer nerve fibres is of course quite possible, but for the present I prefer to discuss the bearing of what I have myself been able to demonstrate conclusively, rather than to reason upon the observations of others, more especially as the facts I have adduced have in nearly all cases been confirmed by a number of observations upon different tissues in different animals, and at varying periods of life.

The drawing illustrates the appearances observed in the neighbourhood of one of the prolongations of a solitary pigment corpuscle in a beautiful specimen of the delicate fibrous membrane from the abdominal cavity of the Hyla, or green tree frog. The portion of tissue under observation displayed the various points represented with great distinctness, and though it was magnified by the $\frac{1}{50}$th of an inch object-glass, magnifying nearly 3000 linear, made by Messrs. Powell and Lealand, so thin was the specimen that little difficulty was experienced in getting clear views of every part.

The branch of the pigment cell (*c*) is seen in the central part of the drawing, and the granules of pigment suspended in the transparent fluid material flowing in the tubular cavity are represented. The nerve fibre (*a*) prolonged from the elongated nucleus (bioplasm of the nerve) is seen to divide into two branches, one of which crosses the pigmentary process, while the other pursues a course for a short distance parallel to it. If, as often happens, a fine nerve fibre runs very close to one of the processes for a short distance, and is then lost to view in consequence of passing behind the body of the cell, and is perhaps hidden by a thicker portion of fibrous tissue,

or by another pigment cell, an appearance leading to the conclusion that the nerve fibre became continuous with the cell, and terminated in it, would be produced. Excessively delicate points of this kind cannot be determined unless specimens of extreme tenuity are examined, and mounted in fluid in which their position can be changed. It is physically impossible to spread such very thin preparations perfectly flat in any limpid fluid, and they can only be manipulated with success in a viscid medium like strong syrup or glycerine. In good specimens in these media we may, however, often follow nerve fibres less than the $\frac{1}{100000}$ of an inch in diameter for a long distance. The edges of the delicate nerve fibre often appear sharp and well defined, and at certain intervals the nuclei of the fine fibres forming what I believe to be the terminal or ultimate nerve net-works, or plexuses, are seen. These bioplasts or nuclei are usually situated more or less on one side, so that the greater part of the nerve fibre is placed upon one side of the nucleus. With regard to the nerve fibrils themselves, many (I believe, all) are unquestionably compound, consisting of still finer fibrils, which are arranged according to the same plan as the nerve fibres in the larger trunks. The constitution of these very fine nerve fibres can often be well made out in my specimens (but not in gold or osmium preparations) at the point where one divides into two or more branches (see Pl. VI.). There are few questions more worthy of being thoroughly prosecuted than this, for by studying carefully the course pursued by the fibres in nerve trunks of various sizes, we cannot fail to ascertain facts of the highest importance, which will greatly influence the conclusions deduced concerning the ultimate distribution and arrangement of nerves. This will in time necessarily lead us to more trustworthy reasoning concerning the nature of nerve action, and the precise way in which some other tissues are influenced, governed, or regulated by the instrumentality of nerve tissue, than will be gained by *assuming facts* concerning nerve structure, and then determining what according to the idea advanced must be the nature of nerve action, as some who have been considered philosophical have ventured to do.

II.—*Report on Slides of Insect Scales.* Sent to the Royal Microscopical Society by the CHEVALIER DE CERBECQ, accompanied by a letter. Examined by HENRY J. SLACK, Sec. R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Jan.* 3, 1872.)

THE objects on slides sent by the Chevalier appear to be mounted in balsam, by which many are rendered so transparent as to require great care in illumination. His intention in forwarding them was to show that they confirmed the idea that a beaded structure was a reality, and not a mere optical illusion. On several slides the scales are arranged in star patterns, so that an illuminating ray from any direction will fall upon them at different angles.

The following examinations were made with Powell and Lealand's new $\frac{1}{8}$th, Ross' C eye-piece and $\frac{4}{10}$ths condenser, stopped to an angle of 75°, and employed with a single radial slot giving unilateral light, generally sent across a scale nearly at a right angle to its long axis.

? DEILEPHILA ELPENOR (Elephant hawk-moth) exhibits various appearances according to illumination and focussing. An aspect of coarse beads is evidently an optical illusion, and even if the objective is well corrected, a false appearance is seen if the focussing is too near the object. The most probably correct view seemed that of longitudinal ribs, with trough depressions between them, each trough crossed thickly with very numerous rows of beads in fine lines. A false aspect is presented if these horizontal beads are not well displayed.

MELANIPPE.—1. A scale showing better than most, gave appearances somewhat similar to the preceding, but the bead rows between the ribs inclined to be convex. This was not the case in some other scales. Some scales appeared to contain a great many more beads than others. In injured scales some bead rows were displaced without injury, but in some spots which looked crushed —no structure visible. Many scales show beading as plain as possible. The fineness of the beading varies in different scales.

2. Delicate beaded ribs, transverse rows as if in depressions; very distinct.

MORPHO HELINOR.—Structure much the same. Beading very delicate and fine, but beautifully sharp and clear.

MICROLEPIDOPTER.—Ribs not as strong as in most others. Beading very delicate; linear on some scales, in others running in curved and angular directions to perpendicular axis. Beads very distinct, though small.

Junonia (garovium ?).—Beaded ribs very distinct; bead rows at right angles to them and between them distinct. A very slight error in focussing confuses the appearances. Each rib should be shown as a raised row of single beads.

Mars changeant.—Ribs numerous; beading fine, but distinct; transverse beading not uniform in angle. Careful focussing needful.

Papilio agaris.—Beading distinct both in broad and long scales; rib beading in former somewhat irregular.

Lycorea atergatis.—Ribs close together; beading distinct. Slight error in focussing confuses ribs and interspaces. Torn scales show that line of separation tends to run between beaded ribs; in some places single bead rows have been detached with little injury.

Abetinor de Surinam.—Rather difficult; beads distinct when well shown, and seeming close together. Damaged scales confirm fact of beading. It is very easy to confound the ribs and interspaces with their bead rows at a lower level.

Atlas China.—No reason to doubt the beading. Ribbing best shown towards serrated tips.

Alcandor (China).—Beading between the ribs irregular. Central ribs in one scale obscured by thick dark irregular beading. Another scale has this character all over.

III.—*On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures.* By W. Carruthers, F.R.S.

IV. *On a Leaf-bearing Branch of a Species of Lepidodendron.*

(*Read before the* Royal Microscopical Society, *Jan.* 3, 1872.)

Plates VII. and VIII.

The beautiful series of specimens illustrating the structure of this *Lepidodendron* have been recently obtained for the British Museum from the valuable collection of Mr. John Butterworth, of Shaw, near Oldham. Like my friend Professor Williamson, I have also to express my great obligations to Mr. Butterworth for the singularly instructive specimens which he has submitted to my examination. Living as he does on the spot where the calcareous concretions, which have supplied such valuable materials to recent workers, occur, imbued with a love for the study of these ancient vegetable forms, and having an extensive and accurate knowledge of their structure, and a quick eye for the parts in their structure which are yet only obscurely known, Mr. Butterworth makes sections with his own hands of carefully-selected materials, in a manner which renders them highly instructive, and greatly facilitates the labour of interpretation and description. The truth of these observations will be apparent by the series of drawings accompanying this paper, made by Mr. Hollick from four slides representing transverse and longitudinal sections of the stem and transverse sections of the leaves close to and at a little distance from the stem.

Of the numerous figures that have been given by Witham,

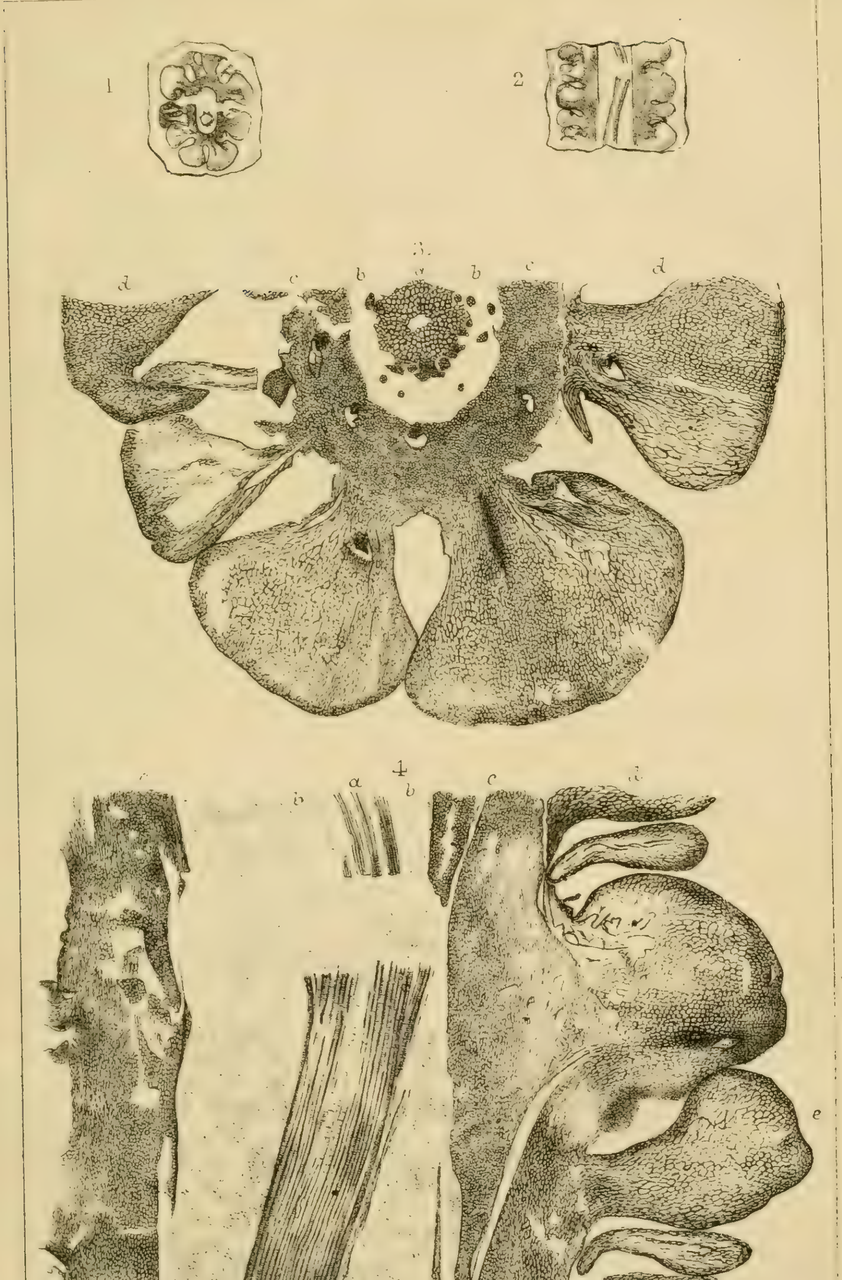
EXPLANATION OF PLATES.

Plate VII.

Fig. 1.—Transverse section of the branch of a *Lepidodendron*—natural size.
„ 2.—Longitudinal section of the same—natural size.
„ 3.—The lower portion of Fig. 1 enlarged 8 diameters. *a.* the vascular axis with the processes representing the leaf bundles not yet liberated from the axis, and surrounded by the free leaf bundles. *b.* the space occupied by delicate cellular tissue, now filled with carbonate of lime. *c.* the epidermal tissues, with openings through which have passed the leaf bundles. *d.* the bases of the leaves, showing also the single vascular bundles passing to them.
„ 4.—Portion of Fig. 2, enlarged 8 diameters. The letters indicate the same parts as are represented in Fig. 3. *c* is the leaf base, showing the line of articulation.

Plate VIII.

Fig. 1.—The bases of four leaves cut through near their connection with the stem.
„ 2.—The bases of the leaves at a greater distance from the stem.
„ 3.—A single leaf (*a.* from Fig. 1), magnified 18 diameters, showing the single vascular bundles at the lower part.
„ 4.—A single leaf (*a.* from Fig. 2), magnified 18 diameters, showing the leaf bundle of Fig. 3 split into two distinct bundles.



A.T. Hollick. ad nat del

C° imp

Branch of a Lepidodendron from the Coal Measures.

Lindley and Hutton, Brongniart, Binney, and myself, none have shown the structure of the leaf bases, and their relation to the stem, except that which I figured in my first communication to this Society;* but in that figure the single leaf was exhibited in one direction only, and not with that completeness which, by the help of Mr. Butterworth's specimens, I am now able to give.

As, however, the specimen exhibits several differences in its internal structure from the *Lepidodendron* stem which I formerly figured, and to which I have referred, I will describe its structure at length. It is most desirable to have as many types of stem structure, and as many varieties of the same type before us, so that when we come to generalize as to their affinities, we may fairly estimate the value of the different variations, and intelligently interpret the affinities of these palæozoic stems to the similar structures in existing allied plants.

The differences in the structure of this stem are so distinct from those that have been already published, that it would be quite in accordance with the practice hitherto adopted to give to this fragment a specific name. But as the species in *Lepidodendron* are distinguished by the form and markings on the leaf scars, and as these are at present unknown in this fragment, it is better to leave it in the meantime unnamed, and trust that the continued labours of Mr. Butterworth, or some other investigator, may result in identifying the named form to which this belongs. The practice of recklessly bestowing names on fragments that obviously belong to, but cannot at the time be certainly correlated with known species, which is followed by some students of vegetable palæontology, is not only adding innumerable synonymes to the already heavily-burthened pages of systematic works, but is creating a terrible and a thankless labour for every honest and exhaustive worker. It is, no doubt, a short and ready solution of every difficulty to set aside the labours of previous workers, and it is besides rather flattering to one's self to be the creator of a series of names. But what would be thought of such a practice in any other department of Natural History? Suppose, for instance, it were discovered that we had in this country another *Papilio* beside the Swallow-tail, and that one entomologist got hold of a hind wing and found that it had two tails, and so full of his important discovery he figures and describes his fragment as *P. bicaudatus*, Mihi; another finds a head with the antennæ attached, and these are obviously more club-shaped than the known species, and of course it is *P. clavatus*, Mihi; the body falls into the hands of a third, and it is thick and short and blunt, and easily distinguished from *Machaon*, so it becomes *P. truncatus*, Mihi; the fore wing turns up, and it has got blue lines and spots, and it would be

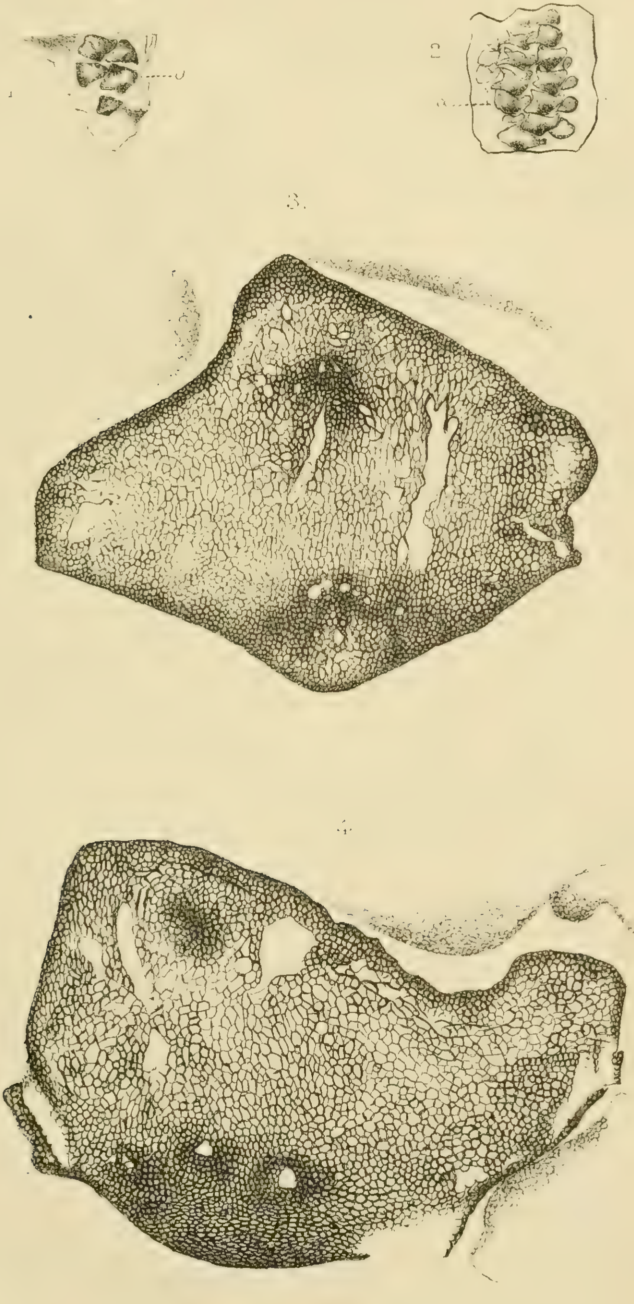
* 'Mon. Micr. Journ.,' vol. ii., Pl. XXVII., Fig. 1 *h*.

absurd not to give this beautiful new species a name, and it is *P. cæruleus*, Mihi; but the body is investigated by an entomologist with an anatomical bias, and he makes some important observations deserving to be published, and the subject must have a name, so it becomes *P. intestinalis*, Mihi; and to terminate an illustration which might be carried to any extent, the caterpillar is found in a field of carrots, a discovery so important must be published at once, and it is *P. Carotæ*, Mihi. The absurdity of such proceedings is apparent from such an illustration as this, but in fossil botany the terrible reality has to be encountered, and not only roots, stems, branches, leaves, and fruit get different names, but different states of the same stem receive different generic and specific names. In the progress of my descriptions I shall take occasion to point out the origin of these accidental states which have received generic positions, and give my reasons for eliminating many worthless names from the science. And I shall further, in the present case, avoid introducing a name which in my next communication I may have good reason for placing as a synonym to a well-known and well-described species.

The axis of this *Lepidodendron* (Pl. VII., Figs. 3 *a*, 4 *a*) is entirely composed of scalariform tissue, which in the transverse section is seen to be irregularly arranged, with the long diameter of the vessels having a radial direction. The ends of the vessels are more or less circular or polyhedral, and they are tolerably uniform in size throughout, except at the exterior, where they are very much smaller. It is important to notice that this central vascular axis is not invested at any part of its circumference by the cylinder of radiating scalariform tissue which I described in *Lepidodendron selaginoides*, Sternb.* The boundary line of this axis is irregular. The small scalariform vessels project, and form recesses in which free bundles of vessels of similar size occur. The projections are the leaf bundles to be given off somewhat higher up the stem. They form as yet only ridges, gradually becoming more prominent, until they are completely separated from the axis, and then they appear as described, as free bundles in a deep furrow. Those bundles are to be found throughout the transverse section of the stem; near the centre they are always cut right across, but as they proceed towards the circumference of the stem, they are cut in a more and more increasingly oblique direction. This is the result of the course the bundles take, being at first almost erect and parallel with the axis, but gradually taking a more outward course until they reach the base of the leaves. They spring from the outer margin of the central vascular axis, a long way below where they pass off to the leaf.

Prof. Williamson, in his recent investigations into the organiza-

* 'Mon. Micr. Journ.,' 1869, Pl. XXVII., Figs. 1 *b* and 2 *b*.



W. West & C.o imp.

Leaves of a Lepidodendron from the Coal Measures.

tion of *Lepidodendron*, proposes to call this axis a medulla.* Our specimen clearly establishes that this cannot be the nature of the axis. It is, as we have said, entirely composed of scalariform tissue; it gives off from its outer surface the vascular bundles which supply the leaves; it has no investing scalariform or woody tissue, and it is placed in the centre of the cellular tissue of the stem. There can be no doubt that it is homologous with the vascular axis of the living *Lycopodiaceæ*, as Witham, in the first paper that treated of the internal structure of a *Lepidodendron*, with remarkable sagacity suggested.† The circumference of the axis is precisely similar to that of *Lepidodendron Harcourtii*, With., being entirely without any investing cylinder; but in that species, as figured by Witham,‡ by Lindley and Hutton,§ and by Brongniart,‖ the interior of the axis is composed of "elongated cellular tissue" (Brongn.), with acute terminations, and having a scalariform structure. There is no indication of such a change in the structure of the axis of this plant either in the transverse or longitudinal section.

The tissue surrounding the axis, Pl. VII., Figs. 3 *b*, 4 *b*, has completely perished in the specimen figured, and the space which it occupied is filled with semi-transparent carbonate of lime, except when here and there a leaf bundle has resisted decay. The destroyed tissue, which is well preserved in some specimens that I hope to figure in due time, was a thin-walled parenchyma.

Beyond this, Pl. VII., Figs. 3 *c*, 4 *c*, are the true epidermal tissues of the stem. The spherical cells have thickened walls. They are of larger size towards the interior of the stem, and the smaller cells of the exterior are found in longitudinal section to be slightly elongated, yet not to so great an extent as in *Lepidodendron selaginoides*, Sternb.¶ This epidermal layer has successfully resisted decay. Scattered through it are several leaf bundles in open spaces apparently completely detached from the tissue itself. This isolation has been produced by the decay of the delicate cellular tissue which accompanied the bundles from the inner perishable layer. Stems frequently occur, especially in arenaceous rocks, in which the hollow interior has been filled in with the amorphous substance of the rock, and this has pushed itself from the interior outwards through the empty passages occupied by the cellular tissue of the leaf bundles. The indurated epidermal tissues having been subsequently converted into coal, and removed, by slow combustion before the specimen was taken from the rock, or by accident

* 'Proc. Roy. Soc.,' vol. xix., p. 500.

† 'Trans. Nat. Hist. Soc.,' Newcastle-on-Tyne, March, 1832.

‡ *l. c.*, Pl. ii., figs. 2 and 4.

§ 'Fossil Flora,' Pl. xcviii., fig. 2.

‖ 'Observ. Sigillaria elegans,' Pl. xxx., figs. 5, 8; xxxi., fig. 4.

¶ 'Mon. Micr. Journ.,' 1869, Pl. XXVII., Fig. 2 *e*.

or atmospheric agency after its discovery, the amorphous cast of the interior with its longer or shorter processes corresponding to the leaf bundles have been supposed to be stems with leaves, and have received the generic name of *Knorria*, and the numerous accidental varieties have obtained a large series of specific designations. The longitudinal section (Pl. VII., Fig. 4) shows one of these empty canals passing upwards and outwards from the vascular axis.

From the exterior of the stem spring the bases of the leaves (Pl. VII., Figs. 1 and 2). There is no cuticle to the stem, as its outer surface is entirely made up of the bases of the leaves. The cellular tissue of the leaves is a continuation of the epidermal tissue (Pl. VIII., Figs. 3, 4). The cells in the interior of the leaf are large, and in longitudinal section somewhat oblong. Towards the margin they are much smaller and more compact, forming a firm surface to the leaf. The leaves proper have fallen from the branch figured, and have left a cicatrix which can be detected in those leaves which are cut through the centre, and to some extent it may be seen in the lower leaf in the longitudinal section, Pl. VII., Fig. 4 *e*. A single bundle passed from the axis into each leaf, and it remained single, for some short distance, through the base of the leaf (Pl. VIII., Fig. 3), but before reaching the articulating surface to which the true leaf was attached, it divided into two distinct bundles (Pl. VIII., Fig. 4). It is important to notice the two appearances that the outer surface of this specimen would exhibit, depending on whether we were examining the surface of the stem or the surface of leaf bases; in the one case the leaf bundle would be single, in the other double.

The leaf bases had a direction nearly at right angles to the supporting stem. They were rhomboidal in transverse section, with the lateral angles acute. It is probable that in both *Lepidodendron* and *Sigillaria* the leaf bases were persistent, and consequently that the principal character on which Corda separated *Lepidophloios* from *Lepidodendron* does not hold good. Indeed the specimen which I have now described should be rather placed in *Lepidophloios* than *Lepidodendron*, were the two designations retained.

IV.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Part III.

Monograph of the European Species.

Plate IX.

About 55 species of Sphagnum are scattered throughout the world, nearly all of which have so much in common in their appearance and habit, that we do not readily observe the characters requisite to establish good species and again to form these into sections.

The structure of the cortical layers of the stem, and the form of the leaves found on the stem, the fruit-peduncle and the divergent branches, combined with the cell-texture of the same, afford the characters on which we must principally rely for specific distinctions, though the last named does not appear to be so invariable as has been supposed, for the spiral threads contained in the cells may partially disappear under certain conditions of growth.

In the arrangement of the species Bridel adopts two sections, Obtusifolia and Acutifolia; and this plan is followed by Wilson in the 'Bryologia Britannica.'

C. Müller, in his 'Synopsis Muscorum,' has *a.* with rounded leaves, *b.* with truncate leaves, and the latter is again divided into two, according as the peduncular leaves have or have not spiral fibres.

Sullivant, in Asa Gray's 'Botany of the Northern United States,' arranges the species by the relative positions of the two kinds of cells, seen on cross-section of a leaf, a character far too minute, and difficult to be observed, to be of practical utility. Prof. Schimper places all the species in two groups, Monoicous and Dioicous; also a most unpractical arrangement, as apart from the inconspicuous nature of the flowers, the species are so frequently found in a barren state, that such a mode of arranging them affords no help to the student. Lastly Prof. Lindberg, in his paper already quoted, which appeared in the 'Öfversigt K. Vetenskaps Akad. Förhandlingar' for 1862, has with a master's hand distributed the Sphagna in natural groups, characterized essentially by the form of the branch leaves, and leaving nothing to be desired.

After separating *S. macrophyllum* as the genus *Isocladus*, Prof. Lindberg divides the Sphagna into two sections, 1, Homophylla having the stem leaves and branch leaves alike in form, and destitute of threads. *S. sericeum* and *S. Holleanum* from Java and Sumatra belong here. 2, Heterophylla having the stem and branch leaves of different forms, and in this section four groups include the European species. In a letter recently received, my kind friend alters the sequence of these, placing *S. cymbifolium*

first, as being the most highly developed form, and this order will be followed here. The drawings have been made by the camera lucida and Messrs. Beck's large instrument, 3-inch object-glass and A eye-piece for leaves, ⅜-inch and B eye-piece for their cell-texture. In the synonymy I have only quoted the more important works, with the hope of being less tedious to the general reader; and in the descriptions, the characters more essentially distinctive of the species, are printed in italics, so as readily to catch the eye and save repetition.

Sphagnum Dill.

Stems dichotomously fastigiate, erect, renewed by an annual innovation at apex; ramuli fascicled at base, flagelliform, partly patulous, partly adpresso-reflexed; the younger clavate, erect, clustered in a dense capitulum at summit of stem. Cauline leaves five ranked, erecto-incumbent or reflexed, soft; ramuline leaves very small at base of branches, thence gradually larger, toward apex narrower and longer, acute, all nerveless, when dry extremely fragile, whitish. Flowers monoicous or dioicous; the male strobiliform on lateral ramuli, antheridia very numerous, solitary at the side of each involucral leaf, on an elongated filiform pedicel, subglobose; female flowers forming long gemmules, lateral, many in each capitulum, archegonia 1–5 in the same perigynium, with very slender, much-branched paraphyses, internal perigynial leaves very small, finally evolved to form the perichœtium and larger than the external. Fruit solitary, up to maturity hidden in the perichœtium, afterwards exserted on a receptacle, prolonged into a pseudopodium. Capsule globose sessile on a discoid turbinate vaginula with a bulb-like pedicel immersed in it; solid, with numerous stomata, chestnut-coloured or blackish; operculum minute hemispherical, mouth naked without any annulus or peristome; columella elevated, hemispheric, covered by the excavato-hemispheric sporangium; pericarpic membrane very thin, whitish, irregularly torn, adhering partly to the vaginula, and partly to the capsule; spores sulphur-coloured or ferruginous.

A. Cymbifolia.

1. *S. cymbifolium* Ehrh.

B. Subsecunda.

2. *S. tenellum* Ehrh.—3. *S. rubellum* Wils.—4. *S. neglectum* Ångst.—5. *S. subsecundum* N. von Es.

C. Rigida.

6. *S. Ångstromii* C. Hartm.—7. *S. molle* Sulvt.—8. *S. rigidum* N. H. and S.

D. Cuspidata.

9. *S. squarrosum* Pers.—10. *S. teres* Ångst.—11. *S. acutifolium* Ehrh.—12. *S. strictum* Lindb.—13. *S. fimbriatum* Wils. — 14. *S. Lindbergii* Schpr. — 15. *S. Wulfianum* Girgensohn. — 16. *S. intermedium* Hoff.—17. *S. cuspidatum* Ehrh.

Of these *S. Ångstromii* and *S. Wulfianum* have not yet been detected in Britain.

Group A. Cymbifolia.—Plants robust, laxly tufted, sometimes intermixed with other species. Branches turgid, those of the coma obtuse; branch leaves imbricated, very broad, ovate, rounded and cucullate at apex, boat-shaped, concave, scarcely margined. Dioicous.

1 Sph. cymbifolium Ehrhart.

Hannöverisches Magazin (1780) p. 235.

PLATE IX.

Syn.—*Sphagnum molle deflexum, squamis cymbiformibus*, Dillen. Hist. Musc. p. 240, Tab. XXXII, fig. 1 (1741).—*Sph. palustre a.* Lin. Sp. Pl. II, p. 1106 (1753). Lindberg Revis. Crit. Ic. Fl. Dan. p. 8 (1871).—*Sph. cymbifolium* Ehrh. l. c., Hedw. Fund. Musc. II, p. 85, Tab. I, fig. 1 (1782). N. Hsch. & St. Bryol. Germ. I, p. 6, Tab. I (1823). Brid. Bryol. univ. I, p. 2 (1826). C. Müll. Syn. Musc. I, p. 91 (1849). Wils. Bryol. Brit. p. 17, Tab. IV (1855). Schpr. Torfm. p. 69, Tab. XIX (1858). Syn. Musc. Eur. p. 684 (1860). Lindb. Torfmos. No. 14 (1862).—*Sph. latifolium* Hed. Sp. Musc. p. 27 (1801). Turner Musc. Hibern. p. 5 (1804). Schwägr. Supp. I, P. I, p. 12 (1811). Eng. Bot. T. 1405.—*Sph. obtusifolium* Ehrh. Crypt. exsic. p. 241. Hoff. Deutsch. Fl. II, p. 21 (1795). Web. & Mohr, Bot. Taschen. p. 72 (1807). Hook. & Tayl. Musc. Brit. p. 3, Tab. IV (1818).

Dioicous. Plants robust, 6–12 inches long, often bipartite, laxly or densely tufted: pale or dirty olivaceous green, sometimes variegated with purple or brownish yellow, pale beneath. Stem solid, woody, tawny brown, sometimes green, covered with a very spongy *cortical web forming* 3–4 *strata, the cells of which are fibrose and porose.* Ramuli 4–5, of which 2–3 are divergent, arcuate, turgid, acute at apex, the others pendulous and appressed to stem, their *cortical cells spiriferous,* and part of them perforated at apex. Cauline leaves lingulate-spathulate, slightly frayed at the rounded apex; very soft, longitudinally sulcate, very laxly areolate, usually without fibres or pores.

Ramuline leaves densely imbricated, broadly ovate, deeply concave with the margins incurved above, *extreme apex cucullate and squamulose at the back by prolongation of the inferior margin of the large pores;* hyaline cells dilated, rhombic, with annular and spiral fibres, pores large, not numerous; chlorophyl cells compressed, entirely enclosed by the hyaline. Male plant like the

female but more slender, the spikelets of flowers thick clavate, green, ochraceous or reddish, the bracts broadly ovate, like the branch leaves in structure. Fruits either clustered on the coma or scattered, peduncular leaves laxly imbricated, broadly oval, obtuse, with their upper hyaline cells fibrose; capsule solid, but little exserted or on a moderately elongated peduncle; spores rufo-ferruginous.

Var. β *squarrosulum* Bry. Germ. Plants slender in loose dark green tufts, divergent branches loose, their leaves more pointed and spreading, somewhat squarrose.

Var. γ *compactum* Schultz Supp. Fl. Starg.

Var. β *congestum* Schp.—*Sph. compactum* Brid. Bry. univ. I, p. 16, pro parte.

Stem mostly divided, densely tufted, pale purple or rufous and green; branches densely crowded, sub-erect, julaceous, obtuse; upper cells of stem leaves having spiral threads.

Hab.—Marshy places in woods, watery heaths and wet hollows in moorlands. Generally distributed. Fr. June. This species is at once distinguished by the cucullate squamulose apex of the branch leaves, and the presence of very fine fibres in the cortical cells. The intermediate leaves are very minute, but may be most readily seen by cutting out a piece of the stem, with a single branch-fascicle attached, and observing them *in situ*. The drawings are made from moderately lax specimens of the typical form, collected by myself on Ben Lawers, β from Continental specimens collected by Herr Schliephacke, γ collected by J. G. Baker, Esq., on Honister Crag.

EXPLANATION OF PLATE IX.

Sphagnum cymbifolium.

a.—Female plant. *a* ♂.—Upper part of male plant.
β.—Var. squarrosulum. γ.—Var. compactum.
1.—Part of stem with a single branch-fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaf. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of base.
6.—Leaf from middle of a divergent branch. 6 ×.—Transverse section. 6 *p.*—Point of same. 6 *a.*—Areolation about half-way up.
7.—Intermediate leaves from the base of a divergent branch.
8.—Leaf from apex of same.
For section of stem and cortical cells, see Pl. XC. in last volume.

R. Braithwaite del. ad nat.

W. West & Co. sc. & imp.

Sph. cymbifolium.

V.—*The Advancing Powers of Microscopic Definition.*

By Dr. Royston-Pigott, M.A. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S.

As it is now above two years since I had the honour of laying before the readers of the Transactions of the Royal Microscopical Society some of the results to which I had been led by several years' patient inquiry, I may now be permitted to state that further investigations have confirmed the views then first broached. During this interval I have had the honour of receiving several communications from distinguished microscopists confirmatory of their truth, and among them I wish to mention that of Colonel Dr. Woodward, who has most kindly forwarded to me three distinct sets of his peerless photographs. Dr. Woodward has favoured me also with the War Department Memorandum on Test Podura, from which I take leave to make a few extracts, as placing the matter in a clearer light than any words of mine can do.

"War Department, Surgeon-General's Office,
Army Museum, *February* 22, 1871.

"For a long time the scales of certain insects have been favourite test-objects for high-power definition. Among these especial importance has been attached to the so-called Podura scale, which has been extensively used by English and American microscopists, as well as by makers of objectives, who have employed it to try their glasses during the process of construction. The market was supplied with slides from various sources, which were offered for sale by all vendors of objects for the microscope, under the general designation of 'Podura scales.' The markings on the scales, as seen by a good 1–5th or by higher powers, were generally described as resembling exclamation marks, without the lower dot, and with a clear line of light running down the mark. It was thought that those objectives were most perfectly corrected, which showed these exclamation marks well defined, with the points sharp, the butts handsomely rounded, the contours black, the central line of light brilliant and broadest towards the butt, and the whole scale as free as possible from chromatic effect." . . . .

"Under these circumstances considerable interest was excited among microscopists by the paper of Dr. G. W. Royston-Pigott, which appeared in the 'Monthly Microscopical Journal' for December, 1869 (communicated May 21, 1869). Dr. Pigott asserted that the exclamation marks were an optical deception, produced by two sets of beads crossing each other at a small angle, each of the exclamation marks corresponding to three or four beads." . . . .

"The photographs (of the *Lepidocyrtus curvicollis*) are taken from a slide presented by Mr. Joseph Beck, partly because the scales on

this slide were the most plainly marked of the series at my disposal, partly because, as Mr. Beck collected the scales himself from a carefully-identified insect, there could be no question as to the species from which they were derived. A brief account of the results of these studies must now be presented."

"1. The *Degeeria domestica* (*Seira domestica* of Sir J. Lubbock). . . . . Their markings were very bold, and the object was in every way a beautiful one. . . . . With carefully-centred illumination the markings resembled the notes of exclamation on the Lepidocyrtus scale. . . . . When suitable obliquity was given to the light, an appearance readily started into view, which recalled at once the description of Lepidocyrtus scale given by Dr. Pigott. In focussing upon the upper surface of the scale, varicose ribs, *which might almost be described as rows of beads, were seen*, with similar ones belonging to the lower surface appearing less distinctly in the intercostal spaces. By depressing the objective still further the lower varicose or beaded ribs came into view, the upper ones appearing less distinctly in the intercostal spaces. These varicose ribs were alternately reddish and greenish, their colour varying with changes in the illumination. When the upper ones were reddish, the lower were greenish, and *vice versâ*. The words varicose ribs would seem to describe the appearance more accurately than 'rows of beads.' Certainly no such distinct isolated bosses were seen, as may be observed, in so many diatoms. It is worthy of note, moreover, that on most of the scales, when the objective, after having been carefully focussed, first upon the upper ribs and then on the lower, was still further depressed, the exclamation marks started into view. . . . . When monochromatic sunlight was used to illuminate the object, the form of the varicose ribbing appeared unaltered, if anything, rather more sharply defined than before; simply the colour phenomena disappeared, the object appearing like a black engraving on a ground of the tint employed. . . . . The photographs were taken with the immersion 1–16th of Powell and Lealand.

"2. The *Lepidocyrtus curvicollis*. With the 1–8th and 1–16th Powell and Lealand, and oblique light thrown lengthways along the scale, appearances were observed which corresponded essentially with what was seen on the Degeeria. The varicose ribs, however, were finer, and as it were more difficult to obtain appearances, which would suggest the epithet beaded. Nevertheless the structure in question was observed in a tolerably satisfactory manner with all the slides of Lepidocyrtus belonging to the Museum. A well-marked scale, on the slide presented by Mr. Joseph Beck, 1–160th of an inch long, and 1–700th broad at its widest part, was selected for the purpose of preparing the photographs which accompany this memorandum. One of these shows the exclamation marks as seen with central illumination, the other shows the varicose or beaded aspect as displayed by oblique illumination."

"The general conclusions arrived at during the investigation may be stated as follows:—

"The scales of *Degeeria* (*Seira*) *domestica* and *Lepidocyrtus curvi-*

collis are very similar in structure, the markings on the Lepidocyrtus being the finest. These scales probably consist of a double membrane, and the markings are probably corrugations in these membranes. The corrugations on the opposite sides of the scales differ in boldness, those on the side next the insect being most prominent. If this side is towards the objective the exclamation marks are seen as the objective approaches the scale, before the varicose ribbings of the two membranes come distinctly into focus. In the reverse position of the scale the varicose ribbings are first seen, and the exclamation marks do not become distinct till after the scale is focussed through. The corrugations appear to have the form of narrow, longitudinal somewhat wavy ribs, which are constricted at short intervals, giving them a varicose aspect (Dr. Pigott's 'beads'). The ribs on the opposite sides cross each other at a small angle, and in this circumstance, and the different degree of prominence belonging to the markings of the opposite sides, the true explanation of the exclamation-mark appearance must probably be sought. These exclamation marks are probably illusive or spurious appearances, notwithstanding the great distinctness they attain under favourable conditions of illumination."

The photographs, the Colonel informs us, represented a magnifying power of 3200 diameters; central light showing the exclamation markings, and oblique illumination the varicose or beaded appearances.

Our talented and distinguished Secretary, Mr. Slack, who is well known for the strength of his convictions, in the 'Popular Science Review' for January, 1872, recurs to the same subject in an elaborate paper, apparently with the honest intention of setting this matter at rest. "The scales of insects," says this gentleman, "are an ornamental covering, probably of some use in protecting the membrane from which they spring, and on which we find them arranged like tiles on a roof; but the flying power does not seem impaired when numbers of them are rubbed off. By their beautiful aspects, they make, according to Mr. Wallace's observations, males and females mutually attractive, and they are frequently the means of disguises which enable their possessors to escape notice of their enemies." He goes on to remark that

"Among the most difficult to be shown clearly, are certain markings on the scales of insects popularly termed Podura; and since Dr. Pigott affirmed that in the famous test-scale now named *Lepidocyrtus curvicollis*, with sufficiently corrected glasses a distinctly beaded structure was to be seen in them, fresh discussion as to their real nature has gone on without ceasing, and strong feelings, as well as reasonings, have been shown by many who had perfectly satisfied themselves with the appearance of the well-known note of exclamation marks, so well shown and so beautifully figured by the late Richard Beck."

He also proceeds to observe that

"An intermediate membrane (in these scales) has been described by some observers, but this appears only the result of a deposit which in most scales takes a more or less beaded form, and may combine into a distinct layer of some kind. Dr. Pigott's 'beads' are by no means inconsistent with the existence of corrugations; and the exclamation marks are probably *true aspects* with a particular focussing and illumination, though few observers, who have taken much care in the investigation, have for many years supposed them to afford an accurate and complete idea of structure. The extreme delicacy of the Podura or Lepidocyrtus scale gives rise to so much difficulty, both of observation and interpretation, that it is advisable to be guided by analogy drawn from easier scales in its interpretation. This plan was pursued by the writer (Mr. Slack), who traced what seemed to be real beads in ordinary and easy butterfly scales, through more difficult ones, up to those of *Lepidocyrtus curvicollis*. Mr. McIntire took up the question with great skill, and with an absence of prejudice somewhat remarkable in a discussion which has excited an unusual amount of strong feeling; and whatever ultimate conclusion may be reached, his observations and beautiful sketches will have a permanent value.* . . . . In scales of *Polyxenus lagurus* he found what was 'very uncommon'; according to his observations, 'a deposit between the membrane' and the scale was a very solid structure. Most of his endeavours to detect beaded deposits led him to think such appearances were only 'ghosts'; and it is well known that false appearances of beading are easily produced under certain conditions. Mr. McIntire's account of his observations and experiments scarcely warrants his conclusions, for he admits 'pigment granules' in scales such as *Analthusia Horsfeldii*, figured long since by Mr. De la Rue, and in some others. Lieut.-Colonel Dr. J. J. Woodward, of the U. S. Army, employed his well-known skill in photographing Podura scales; and in a paper read before the Royal Microscopical Society 'On the Coarser Degeeria Scale,' he says, 'I had no difficulty in making out appearances, which, so far as I can gather from Dr. Pigott's own descriptions and the published discussion of his views, are substantially the same as those seen and shown by him . . . . and even on the more minutely marked and difficult Lepidocyrtus scale I have been able to develop appearances which seem to be substantially similar.'† Dr. Woodward did not, however (writes Mr. Slack), pronounce any decided opinion as to the real structure, but since this paper he has kindly forwarded to the Royal Microscopical Society and to the writer, photographs of *Degeeria domestica*, beautifully exhibiting a beaded appearance. In a communication to the Royal Microscopical Society, read in May, 1871, Dr. Woodward speaks of Mr. Joseph Beck having shown and left with him a fine Podura slide, showing the note of exclamation marks with remarkable clearness, 'but immediately afterwards, with the same optical combination and magnifying power, without any change in the cover

* See 'Monthly Microscopical Journal,' January 1, 1871, &c.

† April No. of the 'Monthly Microscopical Journal,' 1871.

correction, by simply rendering the illuminating pencil oblique, and slightly withdrawing the objective from its first position, he obtained a negative which displayed the bead-like or varicose appearance of the ribbing more satisfactorily than he had previously been able to do.' A photograph of this appearance may be seen at the Royal Microscopical Society's room." *

The article in question next informs its readers that "Dr. Maddox took up this much-controverted question, and after taking great pains to remove oily matters by chemical solutions, he finally made out a ribbed structure, to which he thought the beaded aspects were due, as fine ribs crossing each other would give that effect. It does not seem, however, that the existence of such structures as Dr. Maddox figures negatives the existence of deposits in a more or less beaded form; nor do the investigations of Mr. Wenham, which prove the reality of surface irregularities more or less corresponding with the exclamation marks." †

An interesting letter from THE CHEVALIER HUYTTENS DE CERBECQ, in the January number, gives evidence of the extending range of our Transactions. It begins with so piquant a sentence, and appears to touch so closely the much-vexed question so long at issue, that I may be permitted to make an observation or two upon it. The CHEVALIER writes:—

"*I think it most important for everyone that he should be able to see through the microscope all that is to be seen, and nothing else* . . . . having studied for some months the scales of many butterflies and moths of European, Chinese, and Brazilian origin, I have been struck by the elegance of the beaded appearance those objects presented. I may thus almost entirely confirm Mr. S. J. McIntire's views: *those scales have almost all of them a beaded appearance, perfectly defined*: but where I differ totally from this most distinguished Fellow of the Royal Microscopical Society is, when he asserts in his paper read the 9th November, 1870, 'that those beads have no real existence *as beads*, but are due to the interference of the rays of light by corrugated membranes; that they are, in fact, *ghost-beads*.' Reasoning upon this hypothesis, it is not at all wonderful that Mr. McIntire should have attributed his failures, when 'he wished but could not call into existence *those almost palpable beads*,' as he calls them himself, so distinctly are they visible, 'to a predisposition of his mind, and to the want of that necessary adroitness in manipulating which everyone knows very well is not always at one's own command.' . . . . I think it was due to an imperfect correction that Mr. McIntire was not always able to see the beaded appearance of the scales. . . . . In confirmation of the present letter, I beg the Royal Society to accept a dozen of my slides of butterfly scales, some of them *selected*." ‡

* "Recent Microscopy," 'Pop. Sci. Rev.,' Jan., 1872.

† 'Pop. Sci. Rev.,' Jan., 1872, pages 11–14.

‡ By the courtesy of Mr. Slack I have been (Jan. 11) favoured with a loan of these interesting slides. The *Rhetenor* (*Surinam?*) presents, *when "the colour test"*

At the time of penning these lines these specimens have not been as yet seen by the writer.

The acumen of the Chevalier in microscopic research is well evidenced by his telling remark that he has proved the real nature of the *Surirella gemma* (so beautifully photographed by Colonel Woodward), "that most beautiful puzzle," for he says "I have seen the dots which this interesting diatom wears. This appearance must be the true one, because, having tried that same diatom with a double reflector obliquely disposed at right angles to each other, I have obtained the same appearances." It is worth recounting that the Chevalier used immersion objectives of Ross $\frac{1}{12}$ and Nos. 9 and 11 of Hartnack, and that Mr. McIntire employed a Hartnack also, which he exhibited to me at my residence about two years ago. The improvements, however, which have been made by Messrs. Powell and Lealand in their objectives, have given a new impulse to minute investigation, and I can speak highly of a new one-fiftieth of an inch immersion objective, expressly constructed for me—the first they have made upon their newly-discovered principles of construction. And this exquisite glass has been employed by me to re-test some of the more remarkable results previously obtained.

Thus, in viewing the fringes of delicate objects, I have observed a better defined outline, the most difficult of all kinds of pure and unalloyed definition. The serrated edges and quill-like projections of the ribs of scales have been more sharply, clearly, and more distinctly cut in a light and dark tracery.

I have also observed the general beading of a variety of scales hitherto totally "impalpable" with ordinary glasses employed in the usual manner. Just as twenty-five years ago ordinary glasses only gave a lined appearance to the then difficult tests, such as the *Navicula hippocampus*, and just as an ordinary $\frac{1}{8}$th now only gives the delicate longitudinal ribs on the scales detached from the gnat's wing, which indeed generally presents a pale structureless smooth

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is obtained, the most beautiful appearance of double-striated beading I have yet been able to observe: judging from a comparison with the beading of the Rhomboides, I should estimate them at 100,000 (one hundred thousand) per inch. They can be readily counted in close contact at the serrated end of the scale. The lines on Nobert's 19th band appear to be about $\frac{1}{200000}$th in diameter. The Rhetenor beading can be distinguished with an old but excellent quarter, or rather fifth of Andrew Ross, 1851 date.

The "Atlas, China," is a magnificent object at 4000 diameters with Powell and Lealand's new $\frac{1}{8}$th immersion.

The *Deilephila Elpenor* is a beaded but accurately-striated scale, so regular as to appear absolutely perfect, and presenting the most delicate molecular structure imaginable.

Many of the scales sent by the Chevalier Huyttens de Cerbecq are formed of longitudinal striated beading, crossed with short double rows of transverse beading, containing about four or five beads in each transverse bar, but much smaller than the longitudinal of which the Deilephila is a most interesting type.

appearance (structureless as regards beading), the 1-50th immersion of Messrs. Powell and Lealand displays a rich profusion of beading.

I have also succeeded in displaying the same results with the Lealand $\frac{1}{8}$th and Wray $\frac{1}{8}$th by means of sufficient amplification and accurate spherical and achromatic corrections.

The faltering steps by which microscopists have during the last twenty years groped their way to their present magnificent results must ever be instructive, as developing the grades by which a dim, difficult, almost desperate definition has given place to a brilliant, certain and satisfactory evolution of previously unknown forms.

The question as it now stands appears to warrant the conclusion that all membranous structures of the scales of winged and also unwinged insects are APPROXIMATELY *striated, the striæ in the two layers being either parallel, inclined to each other, wavy or radiating either in one set or both.* I use the term *approximately* as denoting the first approximation attainable to ordinary definition. As the definition advances, these striæ show in different planes, become ribs; and most probably from either diffraction or polarization or both, being in different planes, exhibit complementary colours *red* and *greenish blue*, according to the compound character of the illuminating ray, or if monochromatic light be employed, then shaded more or less. The next step in a more advanced definition is the resolution of these ribs and intercostal spaces into beads, molecules, or agglomerated particles. When these lie in different planes, and the corrections are brought to the utmost perfection at present attainable, they assume respectively different colours, usually ruby or pink, red and sea green, or sapphire blue. The assemblage of these minute little ruby and sapphire like bodies, densely crowded within the ribs (if not actually forming them), is one of the prettiest autographic pictures (of what the microscope may yet be expected easily to perform with proper corrections) with which I am acquainted. Indeed, I may perhaps be excused for venturing the opinion that had the marvellous glasses of to-day been employed a few years ago, many of the microscopical papers (innocently contributed) would never have been penned; at this moment they seem but a monument of transitional science.*

This paper would be extremely incomplete were I to omit to

* For the information of the Fellows who may be disposed to order Messrs. Powell and Lealand's 1–50th immersion objective (price 30 guineas), I may state that it is incumbent to use a covering glass of about 3–1000ths of an inch thick: but that I have found the very clear talc formerly employed by Pritchard (several of whose slides I possess, as also his wonderful doublets of $\frac{1}{80}$th of an inch focal length) answers remarkably well with the new immersion objective.

It is interesting to observe that these doublets, though only possessing an aperture of a very few degrees, develop the exclamation marks in some extremely old scales of *Lepidocyrtus curvicollis*. *These appearances are therefore* not dependent upon *large* aperture, which appears to me a very significant and striking fact.

notice more in detail the performance of Messrs. Powell and Lealand's new 1–16th objective, which they kindly placed at my disposal for a fortnight. This lens is similar though perhaps slightly superior to the celebrated immersion 1–16th signalized by Dr. Woodward as the only one which resolved Nobert's 19th band so as to be counted, and photographed the *Amphipleura pellucida.*

As I had announced in May, 1869,* a double row of spherules in diatoms (when only one-half had been *suspected*), I particularly wished to confirm this appearance. It is well known that formerly black dots used to be seen *above* the true beading (well figured by Beck). The new 1–16th showed exactly double the number of these dots. In individual spherules of the Formosum, no less than three different colours could be descried upon each boss or bead, and with oblique achromatic light a black shadow in addition. The same appearances resulted from a skilful use of their new $\frac{1}{8}$th immersion. I here beg leave to insert the notes taken May 25, 1871, and dictated by an observer with good powers of vision, and confirmed by myself.

"*Particulars.*—Condenser $1\frac{1}{2}$-in. Ross, obliquity of its axis 30°. No stops or diaphragm. Powell and Lealand's best 1–16th immersion.

"(1) *White beads with red tops*, and green narrow parallel stripes in one direction only between the beads.

"(2) A rather deeper focus, *white beads and red beads;* fainter stripes between: the dots form diamond patterns.

"(3) Focus deeper still. Prominent white beads; black shadow on the left and on bottom of the bead (lower side). There is a reddish background, the right-hand side of the bead is green.

"(4) Obliquity of axis of condenser reduced to 10°. Green and white beads, alternate rows, most distinct without any background whatever, the whole space being entirely covered with closely-packed beading, so that the white set of beads appear in the blank spaces figured in the American photographs."

These are apparently exactly twice the general quantity of beads usually seen, the blank spaces teeming with them. When the obliquity was reduced to zero, or the axis of the $1\frac{1}{2}$ condenser (unstopped) coincident with that of the instrument, the *new* beads came out beautifully distinct under the most careful corrections, as green, black, and white, as follows, representing them by G, B, and W.

G	B	G	B	G	B
W	W	W	W	W	
G	B	G	B	G	B

"Green, Black, Green, Black, forming rows with white between."

* Paper received May 21, 1869.

Jan. 8, 1872. With Powell and Lealand's new immersion $\frac{1}{8}$th I succeeded in showing them with the same direct condenser, in a crowded form, with lines running between at right angles, forming a plaid-like pattern (square, and no longer oblique), exhibiting the double set and no intervals.

In my paper of December, 1869 (received May 21, 1869), I mentioned the double row of beading, but with the $\frac{1}{16}$th Powell and Lealand of 1862 I could not succeed in developing such decided appearances as now described. But the beaded form was so familiar with even their 1862 $\frac{1}{8}$th that there appeared nothing extraordinary in the matter till our late President announced in July, 1869, the renowned hemispheres of the Formosum, as shown by "Reade's Prism" (two months after my paper was submitted to the Council). I have reason to believe that our late excellent and distinguished President was in ignorance of the fact that the writer had the priority. But I even then insisted upon the *double set* now beautifully displayed by the new glasses.

It may be remarked that a plane mirror reflects five or six false images, and even a prism of reflexion which depends upon the truth of its plane surface is a poor substitute for direct unreflected light, which I always employ in the most delicate investigations.

In face of these observations with the best glasses, I presume, in existence (Powell and Lealand's own 1–16th, which I think they are unwilling to dispose of, and their new 1–50th immersion made expressly for the writer), I may make bold to predict that a double set of beading will also be discovered upon the Angulatum, Rhomboides, and other difficult diatoms.

Besides diatoms, however, there are beaded scales of such extreme delicacy as to try severely the observer's patience, as well as the quality of the very best glasses now extant. The finest that I have hitherto been able to resolve are the exceedingly minute markings upon the ribs of the gnat's wing scale, mounted in Canada balsam. Powell and Lealand's $\frac{1}{8}$th new immersion can hardly be said to accomplish this feat, although it readily resolves all the ribbed scales sent over by Chevalier de Cerbecq. Probably, before microscopists will succeed in this resolution, considerable practice will be required upon coarser, yet similarly mounted scales. This remark leads me to make a few observations upon

The Resolution of Ribbed Scales.

The advancing penetration of a microscope of the very highest class now attainable, when all necessary attention is paid to the correction of the aberration of the *eye*, as well as the eye-pieces and objectives of the observer, is in no way better exemplified than in

the breaking up of the most delicately *lined scales* into their component molecules.

It is noteworthy that these were the very first objects used to test improving definition. These lines could be seen by minute spherical lenses, and Mr. Williams, the Assistant Secretary of the Royal Astronomical Society, lately informed me that formerly he was able with lenses of his own manufacture, and which he kindly lent to me, of about 1–100th of an inch full length, to distinguish not only the *lines* of such scales, but even *beads*, at which he says he got thoroughly laughed at in those microscopically benighted days. The process by which definition has advanced seems identically the same now as formerly. Diatoms and scales were at first merely lined objects. In scales, next came a ladder-like appearance between the ribs, just like that figured by Mr. Hogg on the scale of the gnat's wing. An improving definition showed more numerous transverse lines: as the glasses are better made, the intervals between the lines appear molecular: the lines become ribs, and lastly the ribs themselves give up their component beads, formed generally in two different planes. All these scales appear to be made upon one type, *a double* agglomeration of molecular beading, more or less regular in size, and more or less symmetrically arranged. Innumerable examples could be given of this. The finest scales of *Menelaus* (*morpho*) transmit a bluish light, and the ribs yield two sets of beads.

Again, the tufted elongated scales of *Hipparchia Janerii* give up a very beautiful collection of reddish and bluish beads, as close together apparently as grains in a sand heap, the bluish being uppermost.

(*To be continued.*)

VI.—*Microscope Object-glasses and their Power.*

By Edwin Bicknell.

In the 'Monthly Microscopical Journal' for Nov. 1871, I made some "remarks" on Dr. Woodward's "Note on the Resolution of *Amphipleura pellucida* by a Tolles' $\frac{1}{5}$th." In those remarks I said that I considered it nothing more nor less than deception in Powell and Lealand in putting out objectives of that power as a $\frac{1}{16}$th, and that Dr. Woodward was no less guilty of the same in knowingly sending out work done by that objective as the work of a $\frac{1}{16}$th. Mr. Wenham has taken me to task for making that statement, and thinks it hardly fair, and discourteous to the gentleman named. I should not have known the power of the so-called $\frac{1}{16}$th, if Dr. Woodward had not given the data; and while he was *very* par-

ticular to give the exact power of the Tolles' $\frac{1}{5}$th, he remained silent in regard to the Powell and Lealand $\frac{1}{16}$th.

I applied one of the ordinary rules of arithmetic to Dr. Woodward's figures, and therein, I consider, lies my unfairness. To say that Messrs. Powell and Lealand did not know the power of the objective when it was made, or that Dr. Woodward did not know its power when he used it, would, I think, be unfair.

However, I thank Mr. Wenham for coming to my rescue. He says: "A scientific microscopist gives the diameters with his illustrations, states the aperture, and the *nominal* power of the object-glass. This quite meets the case." I agree with Mr. Wenham perfectly. Dr. Woodward has given the diameters, and I have applied his own rule; but in this case (the so-called $\frac{1}{16}$th) there was a large discrepancy, amounting to nearly 50 per cent., between the *nominal* and the actual power given. It was very easy to reach the conclusion. I have placed the odium where I think it belongs.

Mr. Wenham further says: "In such a difficult and complex arrangement as a high-power object-glass, it is almost impossible for all the makers to work to the same magnifying standard." I would like to inquire where all the much-talked-of mathematical formulæ are? Does he mean to intimate that the best opticians work by "rule of thumb"? or that it is guesswork? I have been supposing all the time since I became interested in the use of the microscope, and seeing such terms as "index of refraction," "dispersion," "light flint," "heavy flint," and "Faraday flint," "crown glass," &c., that there *were* some mathematical formulæ involved somewhere in the construction of object-glasses; possibly this statement by Mr. Wenham may account for the discrepancy between the *nominal* and the *actual* power of some object-glasses.

I should advise the opticians, if what Mr. Wenham says is true, to wait until the object-glass is done, and then ascertain its power and christen it, and then they would be within a reasonable distance of the true standard. I am well aware that the position of the combinations as determined by the cover adjustment for different objects, will change the magnifying power to a certain extent. I do not see why that range should exceed 15 per cent. of the magnifying power of the objective (unless the object-glass is made like the Tolles' $\frac{1}{5}$th for the U. S. Army Medical Museum, to work both wet and dry with the same front). I have just tried the following object-glasses, *viz.* Tolles' $\frac{1}{2}$, 70°; $\frac{4}{10}$ths, 145°: $\frac{1}{15}$th, 170°: Wales' $\frac{1}{5}$th, 100°: Zentmayer, $\frac{4}{10}$th, 75°: *all of these* are *just on the mark* in magnifying power, and in no case does the power change over 12 per cent. of the actual power of the object-glass, and the three lowest powers will adjust for an ordinary slide bottom up.

Now if we measure the power of an object-glass at its adjust-

ment for uncovered objects, and find that it agrees closely with its nominal power, it will show that the maker means what he says, whether it happens to be $\frac{1}{6}$th or $\frac{1}{16}$th.

Mr. Wenham further says: From an early date $\frac{1}{8}$ths, $\frac{1}{9}$ths, or $\frac{1}{10}$ths, and some now approach $\frac{1}{12}$ths in power (also something about steam-engines). Granted, but does all that prove anything except the truth of my position? Mr. Wenham has in this endorsed my statement a second time, and as far as it being the intention of the opticians to make $\frac{1}{12}$ths at the price of $\frac{1}{8}$ths, and so on, it is simply absurd. In my examinations of objectives of the different makers, I find that Tolles and Zentmayer keep very close to the standard, and I believe English opticians can do the same if they choose.

I wish to ask Mr. Wenham if he does not believe all good opticians *can* put their objectives right on the mark; that is, if they will; and when that is done let *quality* decide the question of superiority?

Museum of Comparative Zoology,
Cambridge, Mass., *Dec.* 26, 1871.

VII.—*Remarks on a Tolles' Immersion $\frac{1}{15}$th.* By Edwin Bicknell.

I notice in the 'Monthly Microscopical Journal,' vol. vi., page 290, a Note on Tolles' Immersion $\frac{1}{5}$th communicated by Mr. Slack, from a letter written by Dr. Woodward to Mr. Slack; the concluding paragraph of which is as follows:—"I wish I could speak as favourably of Mr. Tolles' higher powers. They are very good indeed, but I have *yet to see one of them* which will rival the so-called $\frac{1}{16}$th immersion of Powell and Lealand." I have at present by me a Tolles' $\frac{1}{15}$th which was formerly a dry $\frac{1}{12}$th, and was "converted" into immersion in August, 1868: its power when the combinations are closed is $\frac{1}{15}$th, ang. ap. 170°; when the combinations are opened for uncovered objects its power is under a $\frac{1}{14}$th and its ang. ap. 155°; although somewhat under, I prefer to call it a $\frac{1}{15}$th.

I have this morning, December 25, 1871, by means of sunlight rendered monochromatic by the copper solution, and an achromatic condenser of 145°, stopped off all but the extreme oblique pencil, *resolved, counted, and measured* the three last and most difficult diatoms on Möller's "Probe Platte," *viz. Navicula crassinervis, Nitschia curvula,* and *Amphipleura pellucida:* these are all in balsam. I have a portion of the slide of *Amphipleura pellucida,* which (I believe) Dr. Woodward first photographed (which was

accidentally broken in Boston); this fragment of the cover is so mounted that part of the diatoms are in balsam and part of them are dry. These I have no difficulty in resolving, counting, and measuring, either dry or in balsam. I give here a Table of my measurements, by which it will be seen that the *Am. pellucida* on the Woodward slide is finer than that on Möller's "Probe Platte," and I find it a little more difficult in balsam.

Navicula crassinervis	78,000 lines to English inch.
Nitschia curvula	82,000 " "
Am. pellucida	87,000 " "
" Woodward	92,000 " "

I have seen no account of *Am. pellucida* having been resolved and counted in balsam before. With lamplight and the above condenser I can resolve on Möller's "Probe Platte" up to and inclusive *Nitschia curvula*, the *Am. pellucida* having thus far refused to show its lines.

The objective as I usually use it with the B eye-piece gives about 1500 diameters: when I employ it in counting I use a B *positive* eye-piece and an achromatic concave lens in the draw-tube (amplifier), which just doubles the power, giving me 3000 diameters. It bears this "eyepiecing" well, and by using a Tolles' D solid eye-piece I can still see these fine lines at a power of 6000 diameters; such objects as *P. angulatum* are well shown with this power.

I do not consider this objective one of Mr. Tolles' best resolving objectives. He has made other objectives of both higher and lower power that will excel this one in this respect. This objective is quite as good by central light, has good working distance, adjusts for covers up to $\frac{1}{100}$th of an inch, in fact, according to Dr. Carpenter's comparison, more of a "roadster" than "race-horse."

I make this communication in order that English, Continental, and American observers can compare the above results with their own work.

Museum of Comparative Zoology,
Cambridge, Mass., *Dec.* 25, 1871.

VIII.—*Maltwood's Finder Supplemented.* By W. K. Bridgman.

Maltwood's Finder is unquestionably of considerable service to the microscopist for his own individual use, but it requires the addition of several adjuncts to render it generally useful when required in the way of interchange. Thus, if a marked slide be sent to a friend at a distance, the latter has had, hitherto, no means of ascertaining how

far the indications of his own instrument shall coincide with those of the original observer by whom the marking was effected, consequently it would often happen that "there was nothing in the field." It is, hence, indispensable that he should have some means of determining, not only whether there be any discrepancy, but that he should be able to ascertain the exact amount and direction in which they may differ. This can be effected in a very simple manner. Take, for instance, a common gummed label and draw two lines from one side to the other at right angles so that they cross in the centre, and then at the point of intersection make a minute puncture with a fine needle. Next, place the instrument in position, *with the light central*, and putting on the "Maltwood" bring the central square into view, and let the centre of the square be as near in the centre of the field as possible, so that if a mark were to be made

then it would appear thus, 25 · 25 Now, take a common glass

slip and place it on the stage in the place of the finder, and having wetted the label place it on the glass and move it about until the puncture comes into the centre of the field. If this be accurately done the two ought perfectly to coincide, and therefore when this correspondence is tested under different instruments the variation of distance and direction from where the puncture really appears and where it *ought to be*, will show the different spots at which the same object will be found in each instrument. A test-slide *with the marker's name* accompanying slides sent for inspection will always afford a "key" for their successful examination; or where parties are in frequent communication, or interchanging objects, one might be kept in store by each correspondent.

The accuracy with which this central spot can be determined will, of course, regulate the degree of perfection which may be attained, hence it is desirable to have some mechanical means by which its exactness shall be measured. A very simple pointer may be made in a few minutes by almost any ordinary observer. From the B eye-piece unscrew the upper portion so as to expose the stop in the focus of the eye-lens. With a pair of compasses mark a circle on a thin piece of stiff card which, when cut out, will just *fit firmly* into the tube of the eye-piece, then mark another circle *a little larger* than the opening of the stop, and draw a line through the centre quite across. Next cut out the central portion and fasten down the ring on to a piece of white paper by two pins just above the line, and complete the line across from one to the other on the paper on to which the ring is pinned. A point midway of this line will now be the exact centre of the ring, and it only remains to fasten

some translucent substance which shall mark this spot and not be in the way of observation.

A thin transparent bristle out of a new tooth-brush I have found to answer the purpose very satisfactorily. Selecting one of the finest and straightest, cut a fine notch across the ring in the direction of left to right, and placing the hair in the notch with its inner end a little short of the centre, fasten it down at the other extremity with a small piece of gummed paper, and when dry place it in the eye-piece on the stop and replace the eye-lens as at first. Its accuracy may now be tested by rotating the eye-piece in its place, when the end will describe a very minute circle, in the centre of which is the real centre of the field.

My own pointer is movable. A small milled head is so cut that it turns through only about one-sixth of a circle, and hence at one extremity it brings the hair across the field, and at the other it carries it under the stop to the side so as to be quite out of view when not in use.

As regards the plan of notation, this will probably have been anticipated from the foregoing remarks; for if a dot be sufficient to indicate the centre of the field, it will answer equally well to indicate the relative position of any object in that field, and a dot in any part of a square will serve to note down the precise whereabouts in that square of any object of which it may be desired to preserve a record. Thus, any particular specimen being found, substitute the Maltwood and write down the two numbers contained *in that square over which the end of the pointer rests*, and make a dot in the same relative position to the figures, as $\frac{29}{41}\cdot$, or if it fall on the line, $\frac{29}{41}\Big|$ mark the line as well either vertically or horizontally, but in all other cases the lines may be altogether ignored. The want of some *simple* and *easy*, yet *certain* method of recording, has always been a desideratum, but I find this plan leaves little more to be desired.

IX.—*On a New Micro-telescope.* By Prof. R. H. Ward.

This is designed especially for travelling and field use, but applicable to some of the daily work of the microscopist. It consists of a stand and accessories as follows: An ordinary tank microscope having the body focussed by a rack and clamped at any desired height upon a stand like that of the bull's-eye condensers. Probably few naturalists have any suspicion of the real usefulness of this little piece of apparatus, not only in the study of objects living in an aquarium or preserved in alcohol, glycerine, &c., but for the hasty inspection of herbarium specimens permanently fastened upon

sheets of paper too large for the stage of an ordinary microscope, and for the preliminary examination of objects in jars, boxes, dissecting troughs, &c. The writer always keeps upon his working table such an instrument carrying a double nose-piece (the crooked form) and usually one-inch and three-inch objectives, and uses it continually and with great satisfaction as a substitute for a simple microscope. The brass foot-plate at the bottom of the upright pillar should be made somewhat larger and heavier than usual. A stage of convenient size and simple construction, sliding upon the upright pillar and capable of being clamped securely in any position. This stage carries a diaphragm and mirror below and stage forceps above, and enables the instrument to be used as a vertical compound microscope for ordinary work when, as in travelling or on a field-day, no more commodious stand may be available. This combination may also be used as a dissecting microscope, though for that purpose it is greatly preferable to use the tank microscope as a magnifier only, and to place the object, if opaque, on the table, or if transparent, on the stage of any good dissecting microscope that may be within reach. A draw-tube sliding over the compound body from below, and capable of being fastened by a bayonet catch to the brasswork through which the compound body is moved by the rack. The objective in the compound body now acts as an erector, and another is to be screwed, by means of a large adapter, into the lower end of the draw-tube, to act as the objective. The rack-movement now only varies the power, and may be thus used as a fine adjustment, while the coarse adjustment must be gained by moving the whole instrument. This combination is extremely useful for dissecting, its greatest misfortune being that it is a monocular arrangement. A telescopic object-glass of one-inch linear aperture and four-inch solar focus, to be screwed, instead of the objective, into the bottom of the draw-tube. To this object-glass the compound body with its eye-piece and objective acts as an erecting eye-piece, and is focussed by means of its rack-movement. This combination gives a telescope of good working qualities, and of power entirely disproportioned to its size. A brass pillar about two inches long, having a steel transverse bar for a handle, and at its lower end a gimlet-screw to be fastened into a tree, post, or board. Into its upper end may be screwed the upright pillar of the tank microscope. The gimlet-screw may be made of steel, which is somewhat durable, or a common iron screw may be used, which easily wears out, but can be replaced at a nominal expense. This fixture adapts the instrument to field use as a microscope or telescope.

This instrument should be furnished with a compressor and two objectives,—a one-inch and a two-inch, two-thirds inch, one-half inch, or four-tenths inch, according to the wants of the owner.

Either of these objectives may be used as an erector, though the higher ones will seldom be preferred for this purpose. The four or five inch objective usually furnished with tank microscopes may well be dispensed with, as its effect is easily gained by means of the erecting arrangement. Microscopists who have laid aside the ordinary "erector" of the shops as an entirely unsatisfactory affair, need not, on that account, expect a similar failure in the use of a good one-inch or two-inch objective as an erector.

The cost of this instrument ought not to be large. It should not exceed sixty or seventy dollars to those already supplied with objectives and compressors, or one hundred dollars complete.—*American Naturalist.*

NEW BOOKS, WITH SHORT NOTICES.

The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. 3rd Edition. By J. W. Griffith, M.D., assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and Rupert Jones, F.G.S., Professor of Geology, Royal Military and Staff Colleges, Sandhurst, &c. London: Van Voorst, 1871. Parts I. and II.—We must congratulate Mr. Van Voorst upon his effort to bring out a new edition of the 'Micrographic Dictionary.' It is a work which for years has stood without a rival, and in matter, illustration, and arrangement, it has left little to be desired. Of course, in making these remarks, we are referring to the book at the time of its last edition. Since that time the progress made has been very great indeed, especially as regards the microscope and its necessary apparatus. The work done also both by foreign and English histologists, in the time that has elapsed, has been very great; but that we have hardly to deal with in our notice of the present two first parts of the new issue, as they are really nearly entirely, though not exclusively, occupied with the account of the microscope, of its additional apparatus, of the methods of testing them to see that they are in perfect condition, and of the mode of examining the different objects which the microscopist may have to study.

Now this part of the work is of the utmost importance; for assuredly the Dictionary will be looked up to as an authority, and therefore its information should be full and complete. But we are sorry to have to express our opinion, at the very outset, that this is not at all the case. To this, of course, the editors can very fairly say, "We have given all the information that we consider necessary in the introduction; hereafter, under the special heading, each subject will be dealt with as fully as it merits." And this may be very true, and may possibly explain the deficiency of the information in the first two numbers of the Dictionary. We must say of it that, so far as it goes, it is excellently done. The writing is marvellously clear, and the subjects succeed each other in such order as to be perfectly intelligible. In fact, we know of no work which contains such admirable advice to the student in regard to the many difficulties of microscopic investigation. But when we look to see how far the edition has been influenced by modern work, we confess that the result is not at all satisfactory. For example, we find hardly any reference to the subject of photography in connection with the microscope. The same may be said of that beautiful instrument, the micro-spectroscope, which is dismissed in a couple of lines. And then, worst of all, the question of Diatomaceæ and their structure, and of Podura scales and the scales of Lepidoptera, have been treated of almost as one might who was writing the former edition of the book. These we consider very serious and important omissions from an Introductory chapter to a treatise on the microscope and its work; and though we may hope to see them laid before us in the future numbers, still we must regard it as

an error of omission that they are not given in the first place as an introduction to the student.

We think Mr. Van Voorst would have done well to have had another editor in addition to the three already selected, and that he would have found him easily in the Royal Microscopical Society we have not the slightest doubt—some one who, being constantly at the Society, was well acquainted with the various discoveries which have taken place of late years on the subject of Diatomaceæ, *et hoc genus omne.* And we hope that, if his engagements are not definitively concluded, he will not leave our suggestion without some serious consideration.

As to the contents of the book there is not much for us to speak about; but such as there is, is so far well done. We have gone over this part rather carefully, and we find that the authors have not left unnoticed what has been done of late years, some of their references being to books and papers published as late as 1870. We think the new edition, on the whole, a very fair one; and if the publisher does not absolutely scoff at our advice, we doubt not the future parts will be everything that the working microscopist can desire.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Development of Amblystoma lurida has been recently worked out by Dr. P. R. Roy, an American naturalist. He leaves the development within the egg unnoticed, as he says there is no difference between it and the fish, which has been carefully studied already. The tadpole of this salamander emerges from the egg in twenty-five days. April 25, length one-half inch, colour olive, eye-spots visible, two short holders, gills rudimentary. May 5, tenth day, eyes developed, irids golden, colour greyish olive, with three faint transverse bands of a darker hue, gills plumed, no holders, fore legs starting; is now active, and feeds voraciously on aquatic insects. May 25, thirty days from the egg, fore feet tridactylous, consisting of thumb, forefinger, which is greatly elongated, and middle finger a little longer than the thumb. If there should be an arrest of development at this stage, the track would be bird-like. Next, the fourth finger makes its appearance, and, on the hind feet, the fifth comes still later. What is especially interesting is that when the legs or feet have been amputated, which frequently occurs, the operation being performed by those miniature fresh-water sharks, the larvæ of dragon-flies and water-beetles, the development of the toes is precisely in the same order, first the three toes, then the fourth, and on the hind feet the fifth. The gills are now beautifully plumed, and when closed reach to the centre of the entire animal; hind legs starting. June 20, fifty-six days, hind feet developed. As the lungs increase the gills wither and are gradually absorbed, so that by the middle of August

the gills have all disappeared. The time consumed in this wonderful process is a little over one hundred days.

The Affinities of Crinoids.—Professor Agassiz gives the following account of Herr Metschnikoff's recent researches on these animals. He quotes from the 'Bull. Acad. St. Petersburg,' p. 508, February, 1871. The researches are of considerable importance, as they throw new light on the affinities of the Crinoids. Thoroughly familiar, says Prof. Agassiz, with the Pluteus of Holothurians, Echini, Star-fishes and Ophiurans, Herr Metschnikoff commenced the investigation of their earlier stages with the determination of tracing the presence of the peculiar water-system of the larvæ of the other orders of Echinoderms, what had been previously written by Busch, Allman and Thomson, on the early stages of Comatula, giving no data whatever bearing upon the subject. To his surprise he found no such water-system, nor could he trace anything in any way homologous to it; he also discovered that what constitutes the water-system of adult Crinoids, which has always been homologized with the water-system of other Echinoderms, is developed in a totally different manner. In the free-swimming Comatula larva the bag-like digestive sac is the only organ developed, it becomes the digestive cavity of the adult after the larva attaches itself to the ground. He noticed the tentacles as diverticula of the digestive sac in the interior of the larva; these subsequently force their way through to the exterior, at the time when the digestive bag has become further differentiated, and is provided with a mouth opening in the centre of the oval disk, and an anus opening not far from it on the side of the calyx. There is formed at this stage a large cavity which divides into two parts; the upper part, uniting the hollow tentacles at their base, forms the so-called circular canal, while below it, and connecting with it, we have a large cavity forming the perivisceral cavity, a mode of development of the circular ring and of the perivisceral cavity totally unlike that observed in Ophiurans, Starfishes, Echini, and Holothurians. Metschnikoff compares the mode of development of the upper and lower cavity to analogous processes in the embryonic growth of Alcyonella and other Bryozoa; he traces a striking similarity in the structure and position of the digestive organs and tentacles with similar organs of Bryozoa. However that may be, he has shown conclusively that the larva of Comatula has apparently nothing in common with other Echinoderm larvæ; but, says Prof. Agassiz, we must wait for his figures on this intricate subject before we can decide if the position he assigns to Crinoids is true to nature.

Monocotyledons the Type of Seeds.—A paper on this subject having been read by Mr. Thomas Meehan before the American Association at its last meeting, Dr. T. C. Hilgard made the following remarks upon it. He observed that the whole question came back to the laws of phyllotaxy. The very fact of these "genetic" numbers, as he had called them, required the second element to be derived from the first one; as all radial organs must be derived from their predecessors. The fact itself was apparent in the far-too-much neglected phenomena

of cryptogamous *developments* (or "embryology" of authors). The moss-spore proper (apart from the *Chlorospermeæ* as true moss-*spawns*), develops into a true land (or aquatic) Conferva. The latter bears a bud at the ends of its thread-like "prothallium." Each of its cells is generated out of a preceding one. A terminal cell enlarges into a conical leaf. *Out of that leaf* springs the second, at its base. It is in fact only on the supposition of radial organs generating their successors at the side of the *rift*—at the centre—alternating from either border (as in the case of the pod-leaves, producing fertile ovules), that the whole of phyllotaxic phenomena, and of organic numbers in general, becomes explicable. The production of new elements, however, takes place in a very embryonic condition. Cotyledons already formed do not *divide*. Lobes of fissures, folds, &c., of cotyledons are no divisions, but are due to unequal enlargement. New elements are not formed by division, but by sprouting.

Foraminifera of the Chalk of Gravesend and Meudon.—A very important paper on this subject has been contributed to the 'Geological Magazine' for November last, by Dr. W. K. Parker, F.R.S., our President, and Professor Rupert Jones, F.G.S. It should be read by those who are interested in the subject. The authors conclude by expressing the fact that the specimens figured in the 'Mikrogeologie' are for the most part very minute, such as lie among the finer débris of washed chalk; whilst those treated of by D'Orbigny were larger individuals picked out by means of hand-lenses from the coarser dust of the disintegrated material. The great difference of size, however, among individual Foraminifera carries but little weight in the determination of species; for the conditions, not only of growth, but of feeding ground, depth of water, and climate affect them so greatly, that a form which may be gigantic in one *habitat*, will be arrested or dwarfed in another, retaining all the essential characteristics of shape and structure which are required for its specific identification.

Peroxide of Hydrogen in Pus-globules.—In a paper read before the Medical Society of Victoria, and published in the 'Australian Medical Journal' for August last, Dr. J. Day calls attention to the value of peroxide of hydrogen in destroying the infectious nature of pus-globules. He has tried it in cases of small-pox with advantage. He shows its action on pus-cells to be extremely active.

Nerves of the External Ear of the Mouse.—Professor Schöbl gives an account in Schultze's 'Archiv'* of the above. He states that he knows no cutaneous structure throughout the entire mammalian series that is so richly supplied with nerves—not even the wing of the bat. And he goes on to say that he has discovered peculiar tactile bodies in it, and a remarkable pale terminal plexus of sensory nerves. If the cartilage of the ear be horizontally divided so that the ear is split into two lamellæ, no less than four nerve plexuses may be seen in each. Lying on the cartilage are the larger trunks, which usually divide dichotomously, and intercommunicate in seven different fashions.

* Band vii., Heft iii.

The second layer, like the first, is composed of medullated fasciculi, of smaller size, freely intercommunicating with each other, and lying immediately beneath the capillaries. The third layer is composed of still finer medullated fasciculi, and is on a level with the capillaries. Small fasciculi of this layer, composed of from two to four fibres, run to hair follicles, and, having encircled each with one or several turns, terminate in a little nervous coil, knot, or glomerulus at its base. The nerve knots are almost perfectly spherical, with a diameter of about 0·015 of a millimètre; and they occasionally include a few ganglion cells in their interior. Schöbl estimates the number of nerve knots at about 12,000 for each ear. Finally, from the last-named nerve plexus a fourth plexus arises, which is composed of pale fibres, and which lies immediately beneath the Malpighian layer. At their points of junction are well-marked nodal enlargements. The plates are exquisitely done, but we fear that their artistic merits are higher than their value as fair observations.

Experiments with Vibrating Cilia.—A very curious set of experiments on this subject is described by Professor Jeffries Wyman, M.D. They appeared in the 'American Naturalist' as long ago as last October, but we have not had space to name them. They are certainly very curious and interesting, and are so well described that our readers will have no difficulty in carrying them out. The first-described experiments are those made in water. For these the gills of Unios and Anodontas are well suited. Their cilia are quite active, and vibrate in such directions, that on the inner gill the motion is *from* the free edge, and on the outer *to* it, facts which the experimenter should keep in mind. If an *inner* gill is cut away from its attachment and laid on the bottom of a flat dish, its cilia acting as legs, it will soon begin to move with its *free* edge forwards, and will in the course of time travel the entire length of the dish. Prof. Wyman has seen a whole gill move ten inches in four hours. Under similar circumstances the *outer* gill will move with its base or *cut* edge forwards. This difference depends, as will be readily seen, upon the fact that the cilia of the two gills vibrate in opposite directions. The result of ten experiments gave the rate of motion of a piece of gill measuring 12 mm. by 14 mm., 6 mm. a minute. If two outer gills are laid with their free edges towards each other they will at once begin to approach, and it frequently happens after meeting that one crawls directly over the other. Another and more striking experiment which shows the reaction of cilia on each other may be made as follows. Fasten a gill to a piece of cork under water, and place upon it a portion of a second gill about a half-inch square. If this piece is so placed that the cilia vibrate in the same direction with those of the gill below, it will remain stationary, or nearly so, since the cilia offer no resistance to each other. If now the upper piece is reversed so that the cilia vibrate in opposite directions, the upper piece will move with double the speed and through twice the distance in a given time that it would with its own cilia alone, for while the lower cilia move the upper piece through a certain space, the cilia of the upper piece also move this in addition through an equal space. A third form of

this experiment consists in placing the upper piece so that its cilia vibrate at right angles to those of the lower. In this case, while the lower cilia tend to move the upper piece from side to side, those of the upper tend to move this lengthwise of the lower. The direction which the upper piece takes is a resultant one, *viz.* intermediate between the two. Then follow the experiments in air. Though the tissues of the gills of Unios and Anodontas are quite soft and incapable of resisting other than very light weights, they will nevertheless carry small disks of paper supporting a bristle, on the top of which is a small pellet of cotton or a flag of tissue paper. In order to show the flag more distinctly, a board painted black should be nailed to the edge of the one on which the gill rests, to make a background. With this precaution the experiment may be seen over a large room. To mark the distance traversed, a pointer of white paper should be set up on the board supporting the gill and at the beginning of the experiment, the end of the pointer brought in contact with the end of the flag on the gill. When left to itself, the disk on the gill with its flag at once begins to move to the opposite side, and the flag is seen to recede from the pointer. The distance traversed may be increased to several inches, by placing two or more gills side by side, the free edge of the first slightly overlapping the cut edge of the second, &c. The mucous membrane from the roof of the mouth of frogs is much more solid than the gills of Unios, and the cilia vibrate with much greater force. Different ciliated membranes exert very different degrees of force, but he has found none better suited for experiments than that just mentioned; especially, when taken from the mouth of the bull frog, which gives a large surface. It has the advantage, too, of keeping up its activity for twenty-four hours or more, after being detached from its natural connections, if only kept cool and moist. For moistening it water answers sufficiently well, but the serum of the blood of the frog is still better. The attention of the writer was first called to the possibility of moving weights much larger than was supposed possible by noticing the ease with which a piece of skin which was accidentally placed upon the ciliated membrane was swept off. By loading the piece of skin with weights the mass moved was found to be unexpectedly large. The author has devised a curious and interesting machine for demonstrating to a large class the amount of work which can be done by these ciliated surfaces. It is described in the article to which we have referred.

The Embryology of Chrysopa and its Bearings on Classification.—This is a most important and valuable paper, which was read before the American Association by Dr. A. S. Packard, jun. At one of the former meetings he dealt with the embryology of Diplax. Now he has taken up Chrysopa. He says he did not observe the formation of the blastoderm, but the blastodermic skin ("amnion") of Chrysopa is of the same structure as in Calopteryx. At the posterior end of the egg the round nucleated cells are crowded together in the same way as in Calopteryx. The primitive band is of the same general form, and floats in the yolk as in Calopteryx, but more as in Aspidiotus, though it rests more on the outside of the yolk than in those

genera, and the end of the abdomen rests on the outside of the yolk, rather than rolled in within the yolk; but that the germ is an endoblast (so far as that condition has any special significance) is shown by the fact that the ventral side of the primitive band points inwards towards the centre of the yolk, as in the Libellulidæ, the Hemiptera, and some Coleoptera (Telephorus and Donacia) in contradistinction to the Phryganeidæ and the Poduræ (Isotoma), in which the germ or primitive band floats entirely on the outside of the yolk. After the procephalic lobes and rudiments of the appendages of the head and thorax have begun to develop, a second moult (visceral layer) of the blastoderm is made, which envelops the head and underside of the body, much as in the Libellulidæ and Hemiptera. At this time the embryo is much like that of the last-named insects. The germ does not revolve in the egg, as in the Libellulidæ, but the head remains throughout embryonic life next the micropyle. At the next stage observed, the appendages of the limbs had appeared, the embryo being situated on the outside of the yolk, the end of the abdomen curved around on the opposite side of the yolk. At this time the inner or "visceral layer," forming a second moult of the blastoderm, envelops the germ, much as in the Libellulidæ, and Hemiptera, and Coleoptera (Donacia). It is evident that this *faltenblatt* of Weismann (or visceral layer of Brandt) is shed at a later stage than the "amnion" proper. This stage corresponds with that of Calopteryx figured by Brandt. At this time the germ of Diplax and Calopteryx (Libellulidæ) floats within the yolk, but this difference he would regard as having no special importance, as in the Hemiptera the germ at the same stage of development rests on the outside of the yolk in Corixa, while in the Pediculina, according to Melnikow's researches, the germ floats within the yolk, and in the Curculionidæ (Attelabus) the germ rests on the outside of the yolk (ectoblast), while that of Telephorus is a decided endoblast, *i. e.* floats in the interior of the yolk. After this period, the embryo of Chrysopa exactly corresponds to that of all the Libellulidæ whose development is known (Agrion, Calopteryx, Perithemis, and Diplax). The embryogeny of Chrysopa is identical, then, with that of the Libellulidæ. What becomes, therefore, of the distinction between the "Pseudoneuroptera" and "true" Neuroptera, insisted on by some of the leading entomologists, since Erichson's day? "Never believing that the differences were great enough to separate the Linnæan Neuroptera into two independent orders or suborders (whichever we may choose to call them), I now ask if embryology does not give independent testimony as to the close alliance at least of the Libellulidæ and Hemerobidæ, even if we go no farther?"

Relation of Brachiopoda to Polyzoa.—In his recently-published memoir on the 'Early Stages of *Terebratulina septentrionalis*,' Professor Morse thus discusses the relations which exist between the above two groups. With propriety may also be suggested a certain parallelism between the leading groups of the Polyzoa and the Brachiopods. We have forms like Lepralia, attached by one region of their shell, this shell being calcareous and exhibiting minute punctures, which have

been compared to similar markings in certain Brachiopods. So among the latter group do we find forms attached, as in Thecidium, and some species of Productus; and generally the articulate Brachiopods might be compared to such forms as Lepralia, while on the other hand, such genera as Pedicellina, with its long, pliant, and muscular stalk, or Loxosoma, with a stalk highly retractile, may be compared to Lingula. The limits or intentions of this paper will not allow any considerations regarding the relations of the Brachiopods with the other groups of the animal kingdom. "I have elsewhere expressed my belief that they are true articulates, having nearer affinities with the Vermes; and in view of the above relations of the Brachiopods with the Polyzoa, it is interesting to remark that Leuckart has for a long time placed the Polyzoa with the Vermes, and in a new edition of the 'Outlines of Comparative Anatomy' Professor Carl Gegenbaur removes the Polyzoa from the Mollusca, and associates them with the Vermes."

NOTES AND MEMORANDA.

The 'Archives de Zoologie expérimentale et générale.'—The first number of this periodical has, we believe, appeared. It is under the editorship of M. H. Lacaze-Duthiers, and the subscription list is 35 francs a year. It is to deal with general zoology, supported by the precise laws of morphology, deduced from the most minute histological inquiries, demonstrated by a long and continued study of evolution, and submitted to the control of experience. In a word, it is to deal with *Experimental Zoology*. It will be a quarterly publication, and its Nos. will appear in January, April, July, and October. Each number will contain from eight to ten sheets of matter, and five or six plates, done either on copper or lithographed, and occasionally coloured. It will have a special part for the review of foreign works. A better editor than M. Lacaze-Duthiers could not have been chosen for the serial.

Field Club Prizes.—The Early Closing Association announces, through Mr. Henry Walker, their Secretary, that they intend giving prizes, among other subjects in microscopy, to London naturalists, &c. We regret to see that the Royal Microscopical Society is excluded from the list of competing bodies. In answer to a communication from us, Mr. Walker replied that he would see whether the Royal Microscopical Society should not be included along with the various other microscopical societies, but as we have not heard from Mr. Walker for some time, we fear his efforts were unavailing. The Countess of Ducie is the lady who has given the funds for the establishment of the prizes, which are as follow:—5*l.* 5*s.* for the best List of the Ponds and other aquatic resorts, within fifteen miles of London, and the Microzoa found in them, in the twelve months

between ——— 1871, and ——— 1872, giving the locality of pond, the date of the visit, and the state of the weather at the time. 3*l.* 3*s.* for the second best ditto; 2*l.* 2*s.* for the third ditto. Total, 10*l.* 10*s.* The papers on Microscopy must not in any instance exceed in length two columns of 'The Times' newspaper Parliamentary Debates. Professional collectors and dealers are excluded from the competition. The prizes are intended exclusively for those with whom Natural History pursuits are solely the recreation of their leisure after business hours.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *January* 3, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, and a vote of thanks given to the respective donors.

The Secretary announced that among the donations there were two volumes from the Lisbon Academy, the one containing a description of terrestrial and fluviatile mollusks of the island of Madeira, with papers of an anatomical nature, some descriptions of crustacea, &c.; the contents of the other volume being purely medical.

It was moved by Mr. Suffolk, and seconded by Mr. Shadbolt, "That Messrs. James Hilton and Henry Perigal be requested to act as auditors in the preparation of the accounts of the Society to be presented at the anniversary meeting."

The Secretary read the proposed list of officers for the ensuing year.

A paper "On the Development of Fungi within the Thorax of Living Birds," by Dr. James Murie, was read.

A vote of thanks was passed to Dr. Murie.

Mr. Slack said he wished to call the attention of the Fellows to one of the presents mentioned at the last meeting. He referred to the box of slides of various lepidopterous scales sent by the Chevalier H. de Cerbecq, which he had selected, as showing that the beaded appearances on the scales resulted from positive forms of a globular character, and not from any optical illusion. He (Mr. Slack) had carefully examined the whole of these slides, and he thought it might stimulate others to the same course if he stated what

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

he had seen. He therefore handed in a paper containing a report of his observations.

The thanks of the meeting were given to Mr. Slack.

Wm. Carruthers, Esq., F.R.S., then read a paper "On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures."

A vote of thanks was then accorded to Mr. Carruthers.

Professor Thiselton Dyer (addressing the meeting as a visitor) said he thought that the statements made by Mr. Carruthers would commend themselves to the Society without any support from him. In regard to one point, however, on which Mr. Carruthers had touched, he might be permitted to say a few words. Mr. Carruthers had the advantage of a wide acquaintance with recent plants; this was, however, far from being the case with all those who had written on fossil plants. Indeed, speaking generally, it was unfortunate for the advance of biology that two great a distinction was drawn between what was extinct and what was recent, and he thought it would be a very great advance if the divisions practically existing between palæontology and the study of recent organisms were entirely overthrown. He should like to see accomplished what had not yet been done, namely, the arrangement in our museums of recent and fossil specimens, whether of plants or animals, side by side, so that they could be studied simultaneously. Moreover the division to which he alluded did not exist merely in our museums, it was to be found in the literature of science also. Hence it happened not unfrequently that we meet with vegetable palæontologists still using terms and expressions in their descriptions of structure which were altogether obsolete in the eyes of botanists. For example, Prof. Williamson, at the last meeting of the British Association, proposed to approximate the classification of the vascular cryptogams to the division of flowering plants into exogens and endogens. But this division was now almost universally admitted to be untenable. In his endeavour to correlate the extinct vascular cryptogams with endogenous and exogenous flowering plants, Prof. Williamson was making use of distinctions which had no place in modern botany. Our great English naturalist, John Ray, laid the foundations of a natural classification of flowering plants by dividing them into Dicotyledons and Monocotyledons. De Candolle thought these two groups might be characterized more conveniently by the mode of growth of their stem; he substituted, therefore, for Ray's names, those of exogens and endogens. It had, however, been shown that De Candolle's views involved an entire misconception of the mode of growth. By the researches more especially of Mohl, it had been proved that Monocotyledons were really not endogenous at all; they might, indeed, be more properly described as acrogenous. Then, again, exogenous growth was by no means confined to Dicotyledonous plants. Mr. Berkeley mentioned something very like it in a lichen (*Ipnea*). It was well known to occur in *Lessonia*, the great sea-weed of the Southern oceans, and according to Ruprecht also, in the allied *Laminaria digitata* of our own shores. In *Dracæna*, an undoubted

Monocotyledon, there were regular concentric zones of circumferential growth. In fact exogenous growth was found in vegetable organisms of very different affinities, and was not wholly characteristic of any one. It appeared to be a provision which was correlate with the general form of the whole mass of vegetative organs, and was forced upon the plant, in fact, as a necessary condition of its mechanical stability. For classificatory purposes the terms exogen and endogen were now almost universally abandoned, and Ray's designations, which were found quite valid, were used instead.

As to *Lepidodendron* and allied extinct plants, he had also no doubt about the existence of an exogenous growth in them. Nevertheless they were undoubtedly cryptogamic. The significance of their exogenous growth was not that they had anything whatever to do with Dicotyledons, but that they were large growing plants with a big branching crown, and hence required a stem strengthened gradually by lateral increase. It seemed a waste of time to trace, in a vegetative adaptation of this kind anything like genetic affinity.

Dr. Braithwaite wished to express the great pleasure he had experienced in hearing the admirable paper which had been read by Mr. Carruthers. He hoped it marked a new era in the work of the Society, and that it might be taken as the type of a series of papers confined to single subjects. We had the microscope, which was the implement or tool of work, but there was little systematic use of it. We wanted details of the growths both in vegetable and animal kingdom; and in consequence there were many portions of structure not at all worked out. If a few workers were to concentrate their intellects, and bring their energies to bear upon some of the minutiæ of vegetable or animal growths, we should have a great many more valuable papers before the Society.

The President said, alluding to the remarks of Professor Dyer, that when Mr. Rupert Jones and himself were working together on the *Foraminifera*, they never drew any broad line of distinction between the recent and fossil forms. In his (the President's) collection, which contained five or six thousand slides of Foraminifera, the recent and fossil forms were all placed side by side. By so doing he found by far the greater number of forms were still extant. One type especially, the *Nodosarian* (an ancient but still common form), ran into such an extraordinary series of forms, that he had shown to Dr. Carpenter and Professor Huxley that *eighteen* genera had been made of that form, and that characters of three genera could co-exist in one single specimen. In regard to putting together a great number of fossils, which have received distinct names, if anyone had seen Dr. Carpenter's Introduction to the Foraminifera he will have found a form called *Dactylopora*. He (the President) had found a clue to *Dactylopora* in some caterpillar-shaped shells from India on the common clam. In examining the French tertiaries he found, besides the caterpillar-shaped forms, others that were perfect rings, and that in these specimens there were two or more rings conjoined. Mr. Rupert Jones and himself brought out a series of papers in the 'Annals'; and when Dr. Carpenter took up

Dactylopora, he (the President) put into his hands a very large collection of shells collected by Sir Charles Lyell, and he (Dr. C.) found still more wonderful forms of this type, supposed to be coral. The whole thing showed how the recent is connected with the fossil, when genus after genus, and species after species are put together into one category, and shown to be essentially one specific type. Dr. Carpenter, by his great skill in *Dactylopora* alone, had produced one of the most beautiful monographs in existence, an exact counterpart of the same thoughtful work in which Mr. Carruthers was engaged.

Dr. Royston-Pigott, referring to the paper of Dr. Anthony, published in the current number of the Journal, said, if an object be carefully illuminated by a central light, not overflooding it, so that all *milkiness* may be avoided, and if the observer took the trouble to insert different apertures of the diaphragms behind the object-glass, some very curious effects would be seen, and the remarkable phenomena obtained by this arrangement were worthy of notice. Dr. Anthony's paper had put him in mind of what he himself had seen in the battledore scale. He (Dr. Pigott) had noticed that if you have a very good $\frac{1}{8}$th, no new features would be displayed, and only the usual rows of beads would be seen; but when the aperture is gradually reduced, it becomes a very remarkable object; for you will not only see the large beads, but thousands of others, which stand up with remarkable precision, so much so that you can see them shaded black, and the whole scale usually only marked with parallel ribs studded all over with beautiful beaded effects. He wished to call the attention of the Society to the fact that, by reducing aperture, you may get some very singular revelations. If there were any photographers present that evening he would appeal to them to confirm the principle laid down by Sir David Brewster, who has said that you cannot get a true picture if you use a very large camera upon a small object. Dr. Pigott then described the distortions which would occur in a portrait where too large a camera was used, and which, according to Sir David's principle, were caused by the mixing up together of all the images caused by different areas of the camera lenses; so that the result of such a proceeding would be that a compound image would be obtained, but not a likeness. To show the portrait correctly, you must diminish the glasses, as nearly as you can, to the aperture of the eyes; and you then photograph the object nearly by parallel rays.

Dr. Pigott then illustrated the principle for which he contended by a description of the effects that would be produced by a number of cameras, all photographing one and the same head, at the same instant. The right ear would be shown by one, the left by another, and so forth. In the same way, if the object-glass be of very large aperture a great many views of the same elevated object under the microscope are all different, yet all these views are mixed up into one compound image. The eye of the observer was receiving rays from every side of a minute object at once, as well as from the top. He was looking at it from innumerable points of view at one and the same instant. But when the aperture was reduced, immediately the

surface of the battledore scale was seen studded all over with shaded granules or beads, which stood up in remarkable relief; the diminished aperture increasing the focal depth of vision.

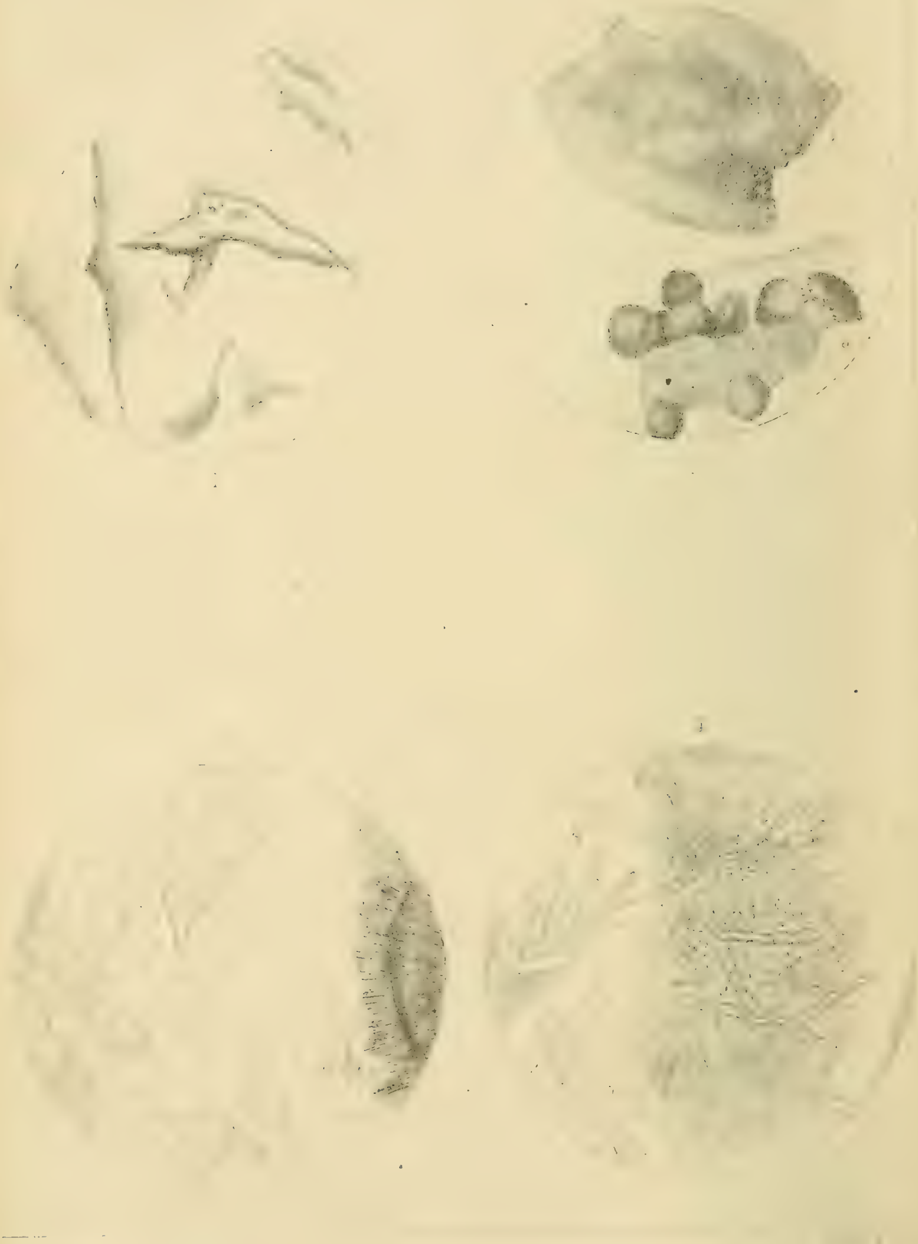
Note.—Dr. Pigott desires to add that he by no means wishes to disparage the value of large angular aperture. Microscopes as well as telescopes, under certain conditions, were however often improved in definition by reducing aperture.

The meeting of the 7th February will be the anniversary meeting of the Society.

Donations to the Library, from Dec. 6th, 1871, to Jan. 3rd, 1872 :—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Journal of the Linnean Society, No. 54	*Society.*
Memorias da Academia Real das Sciencias de Lisboa. Tomo 4. Parte 1 and 2	*Academy.*
Annual Report of the Brighton and Sussex Natural History Society, 1870–71	*Society.*
Journal of the London Institution, No. 9	*Institution.*

WALTER W. REEVES,
Assist.-Secretary.



W.H. Roberts f.

Portions taken from Dr Blanc's sp
Madura foot disease

THE

MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1872.

I.—THE PRESIDENT'S ADDRESS.

(*Delivered before the* Royal Microscopical Society, *Feb.* 7, 1872.)

Gentlemen,

I come before you this evening to deliver the Annual Presidential Address with a mixed feeling. I have a timidity in attempting to represent microscopic lore in what may be termed the new phase, namely, the use of high magnifying powers and great complexity of apparatus. On the other hand, I feel confident in stating that in the use of what one may call antiquated microscopic powers, there is far more yet to be done than many of the Society imagine. High magnifying power is the rage; yet few are the researches but have been foreshadowed by workers in the early part of the present century. I lay much stress upon this point; and were I to refer to an example of one of the highest names connected with the microscope in this country, I should mention one of your early Presidents, John Quekett. I take this man's work as an example of what can be done with low microscopic powers; although it is true that in substantiating detail, or in giving others clearness of conception, as to the intimate structure of tissues, he often illustrated his sketches of texture by highly-magnified objects. The greater number of his discoveries, of which not only the Microscopical Society, but England, may be proud, were made with low powers. This is known not only to myself personally, but to all who knew him intimately.

The microscope, ordinarily, may be said to be applied to the elucidation of fine organisms and minute tissues. It is plain, however, to everyone, that all organs, built up of textural parts, are more easily comprehended by viewing them in their broad phases, than by incessant reference to their component parts. The ordinary embryo, say, of fowl, frog, or fish,—from the size of a mustard-seed to a pea,—can be viewed sufficiently with a hand-lens or a very low power, to elucidate the organic relations of the changing protoplasmic mass. As growth proceeds, it is easy to perceive that each organ evolved necessarily enlarges; and the magnifying power employed might be said to be in inverse ratio to the size of the

object. At length a time comes when it can be no longer viewed with an ordinary object-glass; and to trace its evolution and development, we needs must have recourse to sections. It is mainly with the result of observations made on such *bisections*, as well as of *dissections*, that I wish to occupy your time to night.

The pleasure you have afforded me in appointing me to the honourable post of President has been dashed with a painful sense of unfitness, so little have I lightened the labours of the Council; nor can I be a proper annalist to my fellow-workers. The breathless ardour with which I have followed my own hunt has left me little leisure to give you an account of the country over which I have so hotly ridden. Thomson, speaking of himself, in his 'Castle of Indolence,' says,

"He loathèd much to write, ne carèd to repeat."

I must, however, excuse myself by saying that mine is *secondary*, and not primary, indolence; it is the idleness of labour. An anecdote may illustrate my shortcomings with regard to this unchecked research that leaves neither leisure nor patience for intellectual stock-taking. My kindly task-master, now in Egypt—I refer to Professor Huxley—gave me this scant praise at the conclusion of my long piece of work, the "Shoulder-girdle." "Parker, you're an idle fellow." "Why?" asked I, in astonishment. "Why? because you have made no *abstract*, and have left me to labour through that heavy mass of work, for any and every idea I want to extract from it."

Some recent conversations with "Friends in Council" have made me somewhat more easy about to-night's duty, for they have informed me that I need not speak of things outside my measure, that is, of other men's labours; but that I may give some account of my own.

My own work, Gentlemen, is somewhat ambitious; it relates to the highest types of animal life, and to the crowning part of their organization, namely, the "kingly-crowned head." Not relating, indeed, to all the wondrous structures that are centred there; but to the box holding the precious brain,—the basket carrying the organs of expression and of speech,—and the instruments used in those all-important functions, breathing and swallowing.

Thirty years ago my favourite author in these matters was Sir Charles Bell, a good type of the Paley or *Fitness* school. Such good reverential men loved to trace the handiwork of an infinitely clever *Contriver*, who brought organisms into being in a moment—monads or whales—the plan of each having been perfected beforehand by a mode of thought peculiarly human. Looking from another stand-point, and yet reverent as they, I have no quarrel, even now, with this school. They but saw and understood things

in an *ex parte* manner, pleased with a fitness, delighted with a plan.

There are deeper meanings than these in the vertebrate organization; and as early as 1790 the great poet and morphologist, Goethe, began to get some glimmerings of a gorgeous morphological science unimagined by earlier observers and limited thinkers. In 1807 Lorenz Oken took up, worked out in his peculiar way, and went somewhat mad upon this most fascinating subject. He had several disciples, and the greatest of these was our own countryman, Richard Owen.

I could almost have wished that the "transcendentalism" which grew out of these oblique glimpses, partial and fitful, in the dawn of a new era of biological science, had never reached our shores, and that we had passed direct from the labours of the great "gradationalist," Baron Cuvier, to those of the German Embryologists, such as Baër, Reichert, and Rathke. These men did indeed lay a foundation, deep and wide, by their incomparable labours, for a true morphological knowledge of the Vertebrata.

Socrates was accused of corrupting the rising youth of Athens by questioning the truth of received axioms, and disturbing their unfinished minds with doubts. We may accuse a modern and native philosopher, our English Oken, of misleading students—I speak feelingly for myself—in the opposite direction, namely, by dispelling wholesome doubts by interpreting all the hard sentences of Nature for them, and by holding before their charmed vision an *Archetypal Idea*, instead of setting them to work at the various types, with scalpel, and lens, and microscope.

The inductions of this anatomist were made from particulars far too few on which to build so stupendous a theory; and meantime, the whole life-history of no single type had been worked out.

The facts all fitted, and fitted beautifully; a reason was given for everything; and two laws governed the creation of all the modifications of the Exemplar, namely, homology and teleology; when homology failed, then teleology came to the rescue.

In speaking of this justly-discarded method of morphological research, I am reminded of the words of Bacon in his "Advancement of Science," where he speaks of the "Invention of Sciences."

"The induction which the logicians speak of, and which seemeth familiar with Plato (whereby the principles of sciences may be pretended to be invented, and so the middle propositions by derivation from the principles), their form of induction, I say, is utterly vicious and incompetent: wherein their error is the fouler because it is the duty of Art to perfect and exalt Nature; but they, contrariwise, have wronged, abused, and traduced Nature. For he that shall attentively observe how the mind doth gather this excellent dew of knowledge,

like unto that which the poet speaketh of, *Aërei mellis cœlestia dona*, distilling and contriving it out of particulars natural and artificial, as the flowers of the field and garden, shall find that the mind of herself by nature doth manage and act an induction much better than they describe it. For to conclude upon an enumeration of particulars, without instance contradictory, is no conclusion, but a conjecture; for who can assure, in many subjects, upon those particulars which appear of a side, that there are not other on the contrary side which appear not?

"As if Samuel should have rested upon those sons of Jesse which were brought before him, and failed of David, which was in the field."

Hastening through this part, I must give the *last words* of transcendentalism, as uttered by an anatomist still living beyond the Great Water.

In the number of that useful periodical, 'Nature,' which appeared on the 28th of December last, the editor has solemnly and in good faith given the abstract of a paper read at the Indianopolis Meeting of the American Association for the Advancement of Science, by Dr. T. C. Hilgard. The title of this production is the "Numeric Relations of the Vertebrate System." I can only give you, as specimens, the head and tail of this monstrous production of a human mind—a production which doubtless cost its parent the severest throes, after which he must have "felt himself spent, and fumbled for his brains."

"There are five (not four only) complete neural rib arches to the cranium of all vertebrate animals, to wit—(1) The condylar or sensitive belt, with the condyle plates for side ribs, and the lower arch of the transversely bipartite occiput for its vault piece; (2) the petrosal or acoustic, containing the auditory nerves in its side beams (easily detected by removing the ear drum of felines, &c.), and overarched by the interior belt of the occipital squama; (3) the parietal belt, originally containing the true gustative of fixed tastes (sour, sweet, salt, and bitter), the glosso-pharyngeal, in an incision, from which it is, however, soon crowded out by the internal carotid artery, and the overlapping 'acoustic rib blade.' The next (4) is the optic or frontal, visibly succeeded, in fishes, by (5) the ethmoidal or olfactory vertebra. The rest of the cranium is formed by its 'extremities' or prehensile appendages."

That is the *head* of the theory: now for the *tail*.

"The vertebral blocks, as well as the ribs, are the product of the primitive axial series of (invertebral) discs, which, when completely arrayed, each bear five branches—viz. two pair of haemal arches, two pair of neural arches, and a fascicle of parallel cleets, so to speak, which being cemented together, both in the front and rear, by the superficial ossification of the discs at either end, are fused into the

block pieces, as found, e. g. in the young hog, the cementing slab covering the big neural rib head likewise, and not only the pentagonal prismatic block. The first disciform ossification we find in the corals, forming cribrose ethmoidal discs, such as the closely set 'sigillate impressions' of the *Astræa*, and afterwards left behind as the coccyx, e. g. of Byathophyllum" (*sic*).

I hope, like Richard Baxter's friends, we shall have no more "Last Words"; but that this dying song of a false philosophy, which has been wafted to us across the Atlantic, will soon cease its echoes; and that our attention may be no longer diverted from honest work.

And now I must acknowledge, with some shame, that the noble band of workers who have laid a sure foundation of vertebrate morphology are, with one or two exceptions, foreigners; mostly Germans. I have mentioned three of these, Baër, Reichert, and Rathke; but I must also tell of Müller, Hallmann, Köstlin, Remak, Vogt, and Agassiz, and more recently Kölliker, Max Schultze, and Gegenbaur. Our great Scotch anatomist, Goodsir, was unfortunately entangled in the meshes of transcendentalism, and his most important paper on the Morphology of the Skull, which appeared in the 'Edinburgh New Philosophical Journal' for 1857, although rich in ideas, is yet a hopeless mixture of observation and speculation. Just at this time, when for us there seemed no hope of native workers who should emulate the Germans, Huxley suddenly arose.

Some of you, I dare say, have seen a small modest-looking paper in the 'Proceedings of the Royal Society,' entitled "The Croonian Lecture on the Theory of the Vertebrate Skull." It was delivered before the Royal Society on the 17th of June, 1858, by Professor T. H. Huxley.

That memoir has been before me continually, and I have read therein day and night; it is a lamp bright with pure, intellectual light; or it may be compared with the mustard-seed of the sacred parable, which, though inconspicuous, is destined to grow into a fair and large tree. I will now give you the author's account of the importance of Development—morphology—as compared with a mere collation of the structure of adult forms—the Gradational Method.

"The study of the gradations of structure presented by a series of living beings may have the utmost value in suggesting homologies, but the study of development alone can finally demonstrate them.

"Before the year 1837, the philosophers who were occupied with the Theory of the Skull confined themselves almost wholly to the first-mentioned mode of investigation, which may be termed the 'method of gradations.'

"If they made use of the second method at all they went no

farther than the tracing of the process of ossification, which is but a small, and by no means the most important, part of the whole series of developmental phenomena presented by either the skull or the vertebral column. But between the years 1836 and 1839 the appearance of three or four remarkable essays, by Reichert, Hallmann, and Rathke inaugurated a new epoch in the history of the Theory of the Skull.

"Hallmann's work on the Temporal Bone is especially remarkable for the mass of facts which it contains, and for that clearness of insight into the architecture of the skull which enabled him to determine the homologies of some of the most important bones of its upper arch throughout the vertebral series.

"Rathke showed the singular nature of the primordial cranial axis, and Reichert pointed out in what way alone the character of its lower arches could be determined. For the first time the student of the morphology of the skull was provided with a criterion of the truth or falsity of his speculations, and that criterion was shown to be Development."—P. 5.

From the same treasure-house I extract the following never-to-be-forgotten axioms:—

"1. The notochord of the vertebrate embryo ends in that region of the *basis cranii* which ultimately lies behind the centre of the basisphenoid bone.

"2. The *basis cranii* is never segmented.

"3. The *lamina perpendicularis* of the ethmoid has the same morphological value as the presphenoid.

"4. The petrosal has the same morphological value as the mastoid; if one is not an integral part of the skull, neither is the other.

"5. The nasal bones are not neurapophyses.

"6. The branchial arches have the same morphological value as the hyoid, and the latter as the mandibular arc.

"7. The mandibular arc is primitively attached behind the point of exit from the skull, of the third division of the fifth nerve.

"8. The premaxilla is originally totally distinct from the palato-maxillary arcade.

"9. The pectoral arch is originally totally distinct from the skull."—P. 53.

Having endeavoured to give honour to whom honour is due, I will now, with permission, speak of my own researches.

On the 28th of June, 1860, I read before the Zoological Society, an abstract of my paper on the Osteology of *Balæniceps Rex*. I was at that time working out the subject *gradationally*. But whilst this was in hand, my friend Dr. John Harley showed me what Professor Huxley had been doing, and how completely he had broken up the idols of the transcendentalists; Professor Rupert Jones at the same time lent me the Croonian Lecture.

The commenced work was set aside; the microscope was brought

out; sitting hens were laid under contribution; and the first chick's head yielded me the "basi-temporals" in a distinct condition. This initial piece of work was highly praised by Huxley. Since then no step has been taken, no observation made, without the microscope; every organ, every tract of cartilage, every fibrous band, and every ossicle, has been subjected to microscopical examination, assisted by proper chemical reagents and solutions.

My friend Huxley himself showed me how to use caustic potash, and glycerine; and another valued fellow-worker, Mr. Henry Power, taught me the value of chromic acid. At the kind suggestion of Dr. Sharpey and Professor Huxley, the Royal Society voted me a valuable microscope. Thus have I been helped and befriended on every side, and to the pleasures of science have been superadded the pleasures of friendship.

Let it not be said, Gentlemen, that scientific men are a quarrelsome tribe, my own experience contradicts such an aspersion; for during all these years there has been to me "neither adversary, nor evil occurrent." My old master, Professor Owen, laughs gently at my weakness in going over to the enemy; he wonders at the folly of the man who can take a Huxley for his "guide"; yet, "although he hath laughed on me, I believe it not."

I hope that my mention of the *Balæniceps* paper will not lead any Fellow of this Society to refer to it; it was written in my younger days, and its composition is so peculiar, that my most intimate friends "hide their minds" when it is spoken of in my presence. Its style is said to be the evolution of a new species of writing, and is supposed to be unconformable with any type known to scholars generally. Professor Huxley thought it sufficiently grotesque to deserve a new specific name of "Parkerian." Eleven years have elapsed since then, and my literary infants still retain a peculiar and somewhat *ursine* form; nevertheless, I have laboured hard to lick them into elegance. Two years after the first paper, it occurred to me to take up the great gallinaceous group of birds developmentally, to be illustrated by collateral embryological work. I had been incited to this by finding that one group of these birds, the Tinamous, were essentially ostriches. That was really a discovery in this country; but Sundevall, the Swede, had already said of *Tinamus*, *Rhynchotis*, and *Crypturus*, "Struthiones parvos referunt!" That which delighted me most, however, was to find that sea-gulls were plovers in their infancy, and became specialized into their own type afterwards. This was a germinant idea, and many of you know what fruit it has borne through the rich manuring given to it by Professor Huxley. The result may be seen in his paper, in the 'Proceedings of the Zoological Society' for 1867, on the Classification of Birds.

Working on, in the way I have described—largely with the micro-

scope,—tracing the structure of the skull in the ostrich tribe,—doing a little side-business on the matter of the shoulder-girdle and breast-bone,—I was at last brought to a stand by my guide, who made me solemnly promise that I would close-in upon the chick, and look at nothing else until that type was worked out. I obeyed; and being already, from the influence of the Foraminifera, an evolutionist, my eyes *would* see evidences for that doctrine of creation. I shall not enter into details; but I will reproduce the conclusions I came to in the paper on the Fowl's Skull, in the 'Philosophical Transactions,' for 1869, pages 803, 804. These views, thus leading to evolutional doctrine, are given in the concluding remarks of the recapitulation, and through the influence of my guide are, I believe, clearly expressed. I wrought much with Professor Huxley in the matter, who is not only clear-headed himself, but is the cause that clear-headedness is in other men. My remarks are as follows:—

"Having thus, with as much expedition and brevity as I have been competent to, run through the ten arbitrary but not unnatural stages in the morphological history of the fowl's head, I would conclude by hinting at the importance of the various isomorphisms displayed by the skull and face of this one type in its stages of growth.

"I have described it upwards, but my long and really anxious labour has been in the opposite direction; the stages were traced from that of the old bird downwards to that of the chick of the fourth day of incubation.

"Whilst at work I seemed to myself to have been endeavouring to decipher a palimpsest, and one not erased and written upon again just once, but five or six times over.

"Having erased, as it were, the characters of the culminating type—those of the gaudy Indian bird—I seemed to be amongst the sombre grouse; and then, towards incubation, the characters of the sand-grouse and hemipod stood out before me. Rubbing these away, in my downward work the form of the tinamou looked me in the face; then the aberrant ostrich seemed to be described in large archaic characters; a little while and these faded into what could just be read off as pertaining to the sea-turtle; whilst, underlying the whole, the fish, in its simplest myxinoid form, could be traced in morphological hieroglyphics."

Since arriving at these conclusions I have gone still farther downwards in search of morphological facts; *down* as to type in the frog and salmon; and *down* in the stages of growth, working out in these fishy types the very earliest recognizable stages of the skull, face, and sense-capsules.

As to the mode in which these organisms have been worked out, I may perhaps say that I have been obliged to train myself, not only to be able to appreciate the whole histology of the matter, but what is still more difficult, to learn the meaning of *masses* of tissues, which form the rudiments of the various parts; whilst, as yet, the

tissues are of an *indifferent* kind. The result of this morphological research appears to me to be the gathering together of a mass of evidence in favour of the theory of evolution. I have already spoken of the stages of the fowl's skull, and what they point to; and I am at present engaged in working out the facial arches and nasal capsules of carinate birds, generally, so that the paper on the Fowl, with this supplementary work, may serve as a memoir on the *Bird's* Skull, and not that on the *Fowl*, merely. What do I find?—that the whole group of existing birds, excluding those half-ostriches the Tinamous, as well as the genuine ostriches, form a mere Order, as neat and compact as the Lacertilia among the reptiles. Once a small distance *above* the ostriches, and the life-tree forks and re-forks, and very few steps upwards have to be made before we arrive at the most accomplished types. From that monster bird, the *Dinornis*, to that feathered bee, the humming-bird, there are but a few and easily-traced stages; and from the ostrich, that plumed camel, up to the most perfect birds of the group—

I.

"The ousel-cock, so black of hue,
With orange-tawny bill,
The throstle, with his note so true,
The wren with little quill,

II.

"The finch, the sparrow, and the lark,
The plain-song cuckoo gray,"—

* * * *

The distance, I say, between that giant and these exquisite types (by way of the tinamou, the hemipod, and the songless Passerines of the New World) is very small indeed. Finally, for I fear you are growing weary, the frog, after having passed its simple and primordial stage, passes into a state between the lamprey and the chimæra, and then skipping past the osseous fishes and tailed Amphibia, foreshadows the mammal in its wondrous metamorphosis.

As to the salmon, the *simple stage* once passed, it gets, even before hatching, on to the level of the sharks and rays; in a week it is a Ganoid, and in a few weeks attains a sub-teleostean type—a condition somewhat below the highest kinds of fish, such as the Percoids.

It is said that proper Teleostean (osseous) fishes are not known below the Chalk; yet the Plagiostomes (rays and sharks) are found with the Ganoids in the Upper Silurian deposits—the Plagiostomes, however, being the lower types. All these things "atone together"; and, as far as we know at present, the life-history of each individual of a high type is a repetition of the evolutional progress in the ascent and modification of the vertebrate forms from the *Beginning*.

II.—*Mycetoma: the Fungus-foot Disease of India.*

By Jabez Hogg, Surgeon to the Royal Westminster Ophthalmic Hospital, Hon. Sec. R.M.S., &c.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 7, 1872.)

Plate X.

The specimen of Fungus-foot disease I wish to bring to the notice of the Society was lately received from Dr. Blanc, of H.M. Indian Army, Surgeon in charge of the Rajkote Hospital. The points of interest in connection with it are, first, the disease, which was found to be confined to the sole of the foot, occurred in an unusually young person, a native of 18 years of age, who had previously enjoyed good health; secondly, soon after coming into hospital Mycetoma was diagnozed by Dr. Blanc, who on submitting a very small piece, which came away in a poultice, to microscopical examination, observed well-defined filaments of the fungus; and lastly, the affected part of the sole of the foot being immediately excised, the wound rapidly healed, and in a short time after the patient was able to leave the hospital cured.

The specimen, preserved in strong spirits of wine, presented a hardened, shrunken appearance when it came into my possession. As this was the first time I had had an opportunity of examining the disease in its earliest stage, I was little inclined to cut it about, and destroy its appearance. A small cut had, however, been made across the slightly prominent and discoloured centre of the mass, as represented in Fig. 1. This opening I carefully enlarged, removing small portions for microscopical examination; but finding nothing except quantities of fat-corpuscles and connective tissue, I dissected away a good deal of the surrounding structures, and came down upon two or three blackish-looking minute spots. With a low power, an inch and a half, and condensed light, I made out a group of globular bodies, exactly like balls of soot, or one of the *Smuts* of the Ustilaginous species; represented at Fig. 2, *a*.

EXPLANATION OF PLATE X.

Fig. 1.—The section from foot: one-half the original size; showing diseased spot.
" 2.—*a*, Nest of soot-ball bodies, magnified 50 ×, *b*, A portion of one of same after boiling in liquor potassæ, pressed out on a slide; exhibiting molecular and resinous matter; magnified 150 ×.
" 3.—Another portion, showing at *c* fungoid threads; magnified 350 ×. *d*, Amœboid bodies and fat-corpuscles, magnified 650 ×. *f*, Fat-globules and crystalline or resinous matter.
" 4.—Another portion, showing well-defined dissepimented cells, imbedded in homogeneous colouring matter; magnified 650 ×. *g*, A detached spindle-shaped thread, surrounded by fat-corpuscles; also magnified 650 ×.

On removing some of these small concretions, which proved to be too intractable for further microscopical examination, I placed them in a test-tube, covered them with liquor potassæ, and subjected them for a few minutes to a boiling heat. A small amount only of the colouring matter was dissolved out, but soon fragments were found to be soft enough to break up on a glass slide. A drop of glycerine solution was then added, and a thin glass cover placed over all. With a power of 350 diameters, I first observed numerous detached fragments of an orange-coloured resinous substance, a number of fat-globules and discoid bodies, with granular matter. On carefully focussing and illuminating the specimen by direct light, articulated filaments were seen imbedded, and slightly projecting beyond the edge of the coloured mass; represented at Fig. 3, *c*. When more magnified these were converted into free loops, not unlike papillæ. The fungus threads were for the most part exceedingly minute; there was a compressed, or fossilized appearance about them, if I may so express it.

On increasing the magnifying power to 650 diameters, these threads were resolved into long, jointed, dissepimented cells; some branching out (represented in Fig. 4) and attaining to a considerable length, while others terminate in an enlarged ovoid head, probably a spore receptacle, containing one or more spores. In others, again, a minute oil-globule apparently occupied the centre; but it is not easy to determine this point, from the large quantity of colouring matter present. A peculiar budding out was noticed in some of the globose cells (represented in Fig. 3, *e*), and a few bodies separated away from the coloured mass, were of a paler colour, and partaking of an amœboid form (Fig. 3, *d*).* These later somewhat reminded me of Haeckel's Leptocytode; from the cytode of which this histologist says: a homogeneous membrane is differentiated from the granular contents; prolongations thrust out, and ultimately becoming a free-moving body, a *Protamœba primativa*. The walls of the threads appear in some instances thick, while those which were separated from the homogeneous matter were exceedingly thin and transparent. Notwithstanding the boiling in liquor potassæ, large quantities of fat-granules continued to float about, and the carbonaceous colouring matter was not nearly removed. The growth in some particulars, save that of colour, appears to partake more of the nature of a confervoid plant in its simple articulated threads and cell multiplication, than of a "truffle-like fungus." But as "one swallow does not make a summer," neither does one examination enable me to write or speak with much authority on fungi. I should prefer, in all examinations of

* Dr. Carter has I believe described, although I have not been able to refer to his paper, "Amœboid bodies," in connection with an advance stage of the fungus-foot disease.

the lower forms of life, to see a hundred examples of the same organism present before asserting anything decidedly about it. I must, however, add, so far as I am able to draw a conclusion from the specimens of the fungus-foot I have examined, that, from the relatively small proportion the fungoid filaments bear to the diseased mass, the fungus can only be regarded as a secondary product; one which may aggravate disease, but can hardly be said to originate it.

III.—*The Advancing Powers of Microscopic Definition.*

By Dr. Royston-Pigott, M.A. Cantab., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S., formerly Fellow of St. Peter's Coll., Cambridge.

Part II.

(*Continued from page* 68.)

It seems unnecessary to describe further the complicated beaded system of insect scales distinguishable by the most perfect glasses now made, notwithstanding the very beautiful specimens sent over by The Chevalier de Cerbecq, which I strongly recommend to the attention of our Fellows. I therefore pass on to notice more particularly "advancing powers of definition" illustrated by Mr. Slack in his interesting paper on the Pinnulariæ, which gives a most valuable confirmation of my views as to the inadequacy of the old-fashioned glasses to deal with the higher order of definition. As I had experienced considerable difficulty in developing the beading of Pinnulariæ with an 1862 $\frac{1}{8}$th of Powell and Lealand four or five years ago, I was pleased to receive a statement from Mr. Slack, that with his new immersion $\frac{1}{8}$th of Powell and Lealand he had completely succeeded in resolving the *costæ* into component beading: at this time I did not possess the new glass. And I very much question whether old-fashioned glasses will resolve them now. Speaking of this beading, Mr. Slack sagaciously observes (even when using the new $\frac{1}{8}$th) in his paper:—

"When the best has been done with any objective, it becomes evident that a slight increase of the difficulty, from greater minuteness of structure, would render it invisible and make the surface look plane."

It is noteworthy that to most glasses the interspaces between the beads of the Formosum and of the *Podura curvicollis Test-scale do look quite plane* and free from structure.

"An examination of the Pinnulariæ on Möller's type slide, led to the belief that the costæ of such diatoms as *P. viridis*, *nobilis*, &c., had been misunderstood. That instead of broad, insolvable ribs, a truer view exhibited fine lines of beads springing from the median band."

Those who wish to succeed, with inferior glasses, to develop Podura and Lepisma beading composing the ribs (corrugations or rumplings seem terms rather indistinct to give a good idea of truly formed ribbings), would probably be assisted by practising the resolution of the ribs shown by the *Pinnulariæ,* and such diatoms as *Surirella gemma.* Mr. Slack also says:—

"P. nobilis.—Very difficult except in lucky parts and valves. . . . Median band beaded in complicated pattern.

"P. PEREGRINA.—Median band and costæ resolvable into rows of beads.

"P. LATA.—Median band consists of rows of fine beads at right angles to costæ.

"P. DIVERGENS.—With D eye-piece the $\frac{1}{8}$th resolves the median band into rows of beads which curve round the expanded ends of the two furrows."

Colonel Woodward kindly informs me that he has not yet been able to see the median bands of either Formosum, Angulatum, or Rhomboides resolved into beads; but after this feat by Mr. Slack, I trust he will be encouraged to produce photographs of the beaded median line of diatoms, notwithstanding they were somewhat confidently denounced as spurious after a statement had been made by the writer in the following words:*—

"In my hands an immersion 1–16th has displayed the beading of which the median ribs of the

Formosum (*a*) (diameter, three to one)
Angulatum (*b*) (diameter, three to two)
Rhomboides (*c*) " "

appear to be composed. Very curiously the diameter of these beads bore a different proportion to the general beading in each case, as above indicated—a confirmatory fact for the truth of their existence, and negativing the supposition of their being spurious, and guaranteeing their integrity."

The Advancing Definition of Lined Objects.

As it may be laid down as an axiom that in all optical glasses there is a residuary spherical aberration of either the first, second, or third order, the very finest glass now made bears to the very finest and most delicate test-object somewhat of the same relation that ordinary glasses bear to the rougher tests.

The ordinary glass showed lines; the extraordinary, beads. So where a splendid glass now only shows lines as in the *Amphipleura pellucida*, a glass of transcendent quality will by-and-by resolve this again into its components.

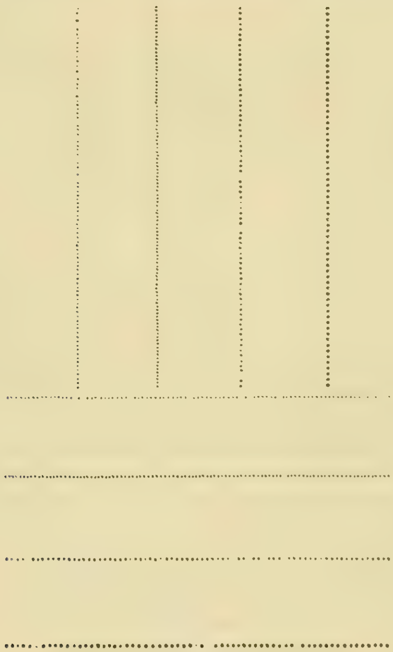
A Tolles' $\frac{1}{15}$th immersion, with monochromatic light, has shown the *Amph. pellucida* with 87,000 lines to the inch, counted by Mr. Bicknell, of Cambridge, U.S., though mounted in Canada balsam. Messrs. Powell and Lealand show a much finer scale than this, without monochromatic light, with their $\frac{1}{16}$th.

A very great source of difficulty is the spurious set of lines which haunt every fine line shown in the best microscopes, even when the corrections are pushed to the finest point of adjustment.

* November number of this Journal for 1870. First described in 'Pop. Science Review,' April, 1870, Plate.

Colonel Dr. Woodward very justly remarks that the causes of these false appearances have puzzled the best opticians of the age. The character of these lines I have observed to vary with different instruments, and in inspecting Nobert's lines, each objective seems to have its own peculiar set of spurious ones.

The great difficulty of resolution presented to the human eye by lines formed of dots is well illustrated by the following diagram:—



If the observer shuts one eye and holds the paper at ten inches, he will find that one of these lines is better resolved at a certain angle of position by turning the paper gradually so as to incline the line more and more to the vertical. This simple experiment illustrates the difficulty of resolving *lines* (really composed of minute dots), into their components. *In Nobert's lines,* of course no component dots exist, but the spurious lines are developed in profusion. The photo-picturing of these lines, however, seems to present less difficulty than diatoms, which with the same degree of fineness probably possess a very complicated structure. The finest *Amph. pellucida* photographed is given, I believe, at

	92,000	lines per inch.
Nobert's XIX Band ..	112,000	,,

Mr. Bicknell says, "With lamplight and the condenser I can resolve on Möller's Probe Platte (Type plate) up to and inclusive *Nitschia curvula;* the *Am. pellucida* having thus far refused to show its lines."* The advancing power of the microscope is demonstrated by the readiness with which Powell and Lealand show these lines with their 1–16th and lamplight. Mr. Bicknell measures his specimen at 87,000 lines only per inch. Powell and Lealand at (I think) about 100,000. But specimens vary very much in difficulty and fineness.

The *Surirella gemma,* ordinarily a line object, gives up beautiful beads. In the photographs presented to me by Colonel Woodward I counted with a pocket lens *thirty beads,* and carefully inserted the points of a pair of fine compasses into the centre of the *first* and *thirty-first* bead so as to give the exact measurement of thirty beads. This I found to be on Photograph xvii

$$1 \text{ inch and } \frac{125}{1000} \text{ or } 1''\cdot125.$$

The power marked on the card was 3100 (probably by a clerical error), which gives

83,000 beads per inch, nearly.

This appears rather too great. Fortunately the Colonel has sent me two photographs of the same diatom. The larger is not quite $2\frac{1}{4}$ times the size † of the smaller. If therefore the magnifying power employed be as stated upon the smaller one, 1034, the second photograph is magnified not 3100 times, but

2200 times,

which gives nearly

59,000 beads per inch.

IV.—*The American Spongilla, a craspedote, flagellate Infusorian.*

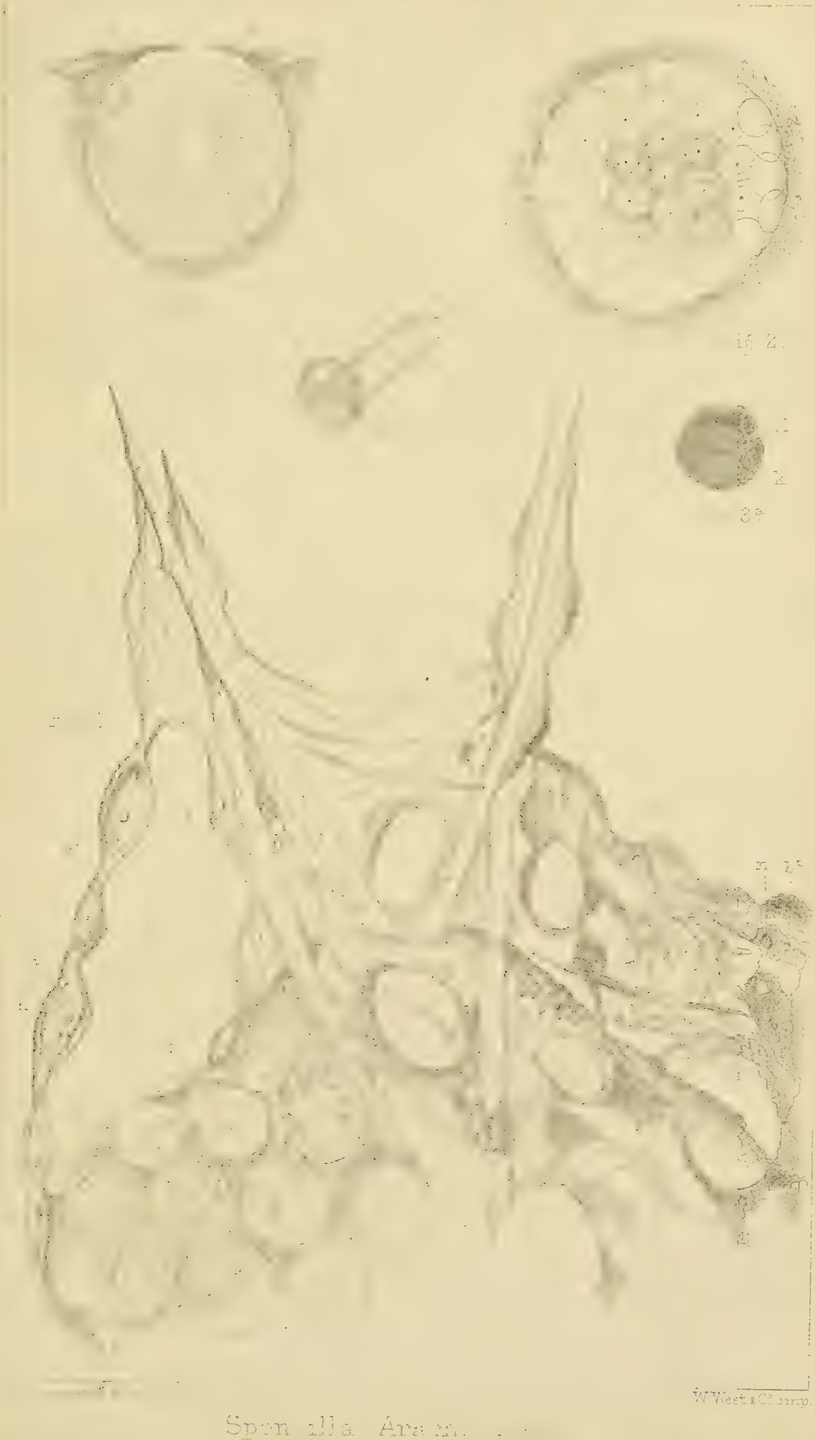
By H. James-Clark, A.B., B.S., Prof. Nat. Hist., Kentucky University, Lexington, Ky.

Plate XI.

The argument of Haeckel, and others, that the Sponges are essentially compound Polypi, is virtually based upon the assumption that the minor (afferent) and major (efferent) ostioles of the former correspond to the mouths of the latter; and that the profusely branching afferent and efferent canals of the Sponges are strictly

* Page 71, No. xxxviii., this Journal.

† Nearly as 347 to 162, by actual measurements.



W. West & Co. imp.

Spongilla Ara...

comparable with similar canals in the polypidom of Halcyonarians: and, by implication, that the cilia-bearing cells of the interior lining-wall of the Zoophyte find their homologues in the ciliated, cell-like bodies of the interior chambers of the Porifera. If, now, it should turn out that these last are not altogether mere cell-components of a tissue, but are each, severally, an independent body, although closely connected with others in a common bond, then the attempted parallelism between the two groups must utterly fail of confirmation. The tendency of Carter's later investigations, and our own too, is to show that this is no vain supposition.

For ourselves, we hold that each *ciliated body* of the Sponge is a *cephalic* member (a *cephalid* in this case) of a polycephalic individual.* We believe, as far as we can understand his undecided, rather hesitating position, Carter's latest decision is, that the Sponge is a community of Amœbous individuals,† and not a

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EXPLANATION OF PLATE XI.

Sponjilla arachnoidea Jas.-Cl.

The following letters apply to identical parts in all of the Figures:—*a*, Investing membrane; outer division.—a^1, Sectional profile of the cytoblastema of *a*.—*b*, Cells in the thickness of *a*.—b^1, Cells (like those at *b*) about the spicules (*e*).—b^2, Cells of the investing membrane, with their nucleus; a surface view.—b^3, Temporary junction (by contact only) of the outer (*a*) and inner (*c*) divisions of the investing membrane.—*c*, Investing membrane; epithelioid inner division, in sectional profile.—c^1, Interspaces between monad chambers.—*d*, Junction of the divisions of the investing membrane along the spicules.—*e*, Larger spicules.—e^1, Smaller spicules.—*f*, Circulatory apartment.—*g*, Monadigerous mass.—*h*, Monad chambers and monad groups.—*i*, Aperture of *h*.—*j*, Monads, or the body proper in Figs. 3 and 3 *a*.—*k*, Cylindrical collar of *j*.—*l*, *Flagellum*.—*n*, Nucleus.—*o s*, Minor ostioles.—*v*, Contractile vesicles.

FIG. 1.—Magnified 320 diameters. Part of a very young *Spongilla*, of an oblate spheroidal form, and about $\frac{1}{25}$th of an inch in diameter. On the right is presented a face view of the investing membrane and the underlying monadigerous mass. On the left the focus is so adapted as to be fixed on a face view of the monad mass, and at the same time on a sectional profile of the investing membrane at a^1, b^3, *c*, and *d*.

FIG. 2.—Magnified 780 diameters. Interior of a monad chamber, seen through the aperture; the monads appear in end view, and crowded together side by side like a pavement work.

FIG. 3.—Magnified 1600 diameters. A single monad, as seen in profile in the monad chamber. Only two contractile vesicles were present in this specimen. The cylindrical collar (*k*) is extended to its utmost.

FIG. 3 *a*.—Magnified 1600 diameters. Foreshortened, front view of a monad; the body (*j*) in the distance; the hollow cylinder (*k*) projecting toward the observer like a dark hoop, and the *flagellum* (*l*) in the centre appearing as a black spot.

FIG. 4.—Magnified 780 diameters. Sectional view of a monad chamber, bringing the aperture (*i*) into profile, as well as the monads which lie at the same level; thus showing their convergence about the central open space.

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* See article on "Polarity and Polycephalism," 'Silliman's Amer. Journal,' January, 1870

† See Carter "On Fecundation in the two Volvoces;" "On Eudorina, Spongilla, &c.," 'Annals and Magazine Nat. Hist.,' January, 1859; also for July, 1871, "On New Sponges, &c."

polycephalic unit. Yet, whichever view prevails, the tendency is the same, and the Polyp theory is negatived most unquestionably. The incompatibility of the interior organisms of the two groups, above mentioned, is so great that it would seem as idle to elaborate a proof of it, as to attempt the demonstration of an axiom. The question is really circumscribed, according to the method of Haeckel, to arguing that, since a system of branching canals in the Sponge reminds one very strongly of the intricate network of passage-ways in the basal parts of certain Polyps, therefore the two are homologous, and bear an identical relation to the rest of the organism Carter* has answered this far-fetched homology with considerable detail in a recent paper; and we do not therefore feel called upon to add more to it.

The principal aim of this article is to furnish new material in proof of the polycephalism of the Spongiæ, and particularly in regard to their relation with the *Protozoa Flagellata.* We are highly pleased to find that Carter has lately† confirmed our earliest observations‡ as to the organization of the collar-bearing monads of *Leucoselenia*, by an investigation of *Grantia compressa.* He has also accepted our interpretation of the horn-like processes of the sponge-cell of *Spongilla alba;* that they are the outlines of a membranous collar in profile.

We have now to bring forward a fourth example of a *craspedote*, flagellate monad-cephalid in a Sponge. It seems to be a *Spongilla*, but specifically, at least in its monads, it differs from the English forms. For convenience sake, we will call it *Spongilla arachnoidea*, from its resemblance to an irregular spider-web. It lives in fresh-water streams and ponds, usually about the bottom of the stems of water-plants, or wherever there is considerable shade; apparently avoiding the light, as we seldom, if ever, found it in open water. In size it varies from a few inches to half a line in diameter; of no definite shape; and has a uniform fuscous or yellowish-brown colour; and is wrapped about by a filmy, transparent, colourless envelope ("*investing membrane*" Carter). The brown colour is inherent to the interior mass, in which the groups of monads are imbedded; in fact, the latter are themselves as strongly coloured by brown granular contents. The "investing membrane" is also slightly tinged with amber colour by the large and small spicules which are imbedded in it. Excepting in very small specimens, foreign matter is often so thickly spread over the surface as to obscure the view and seriously interfere with a correct interpretation of the relation of parts. We have been most fortunate

* "On New Sponges, &c.," 'Ann. Mag. Nat. Hist.,' July, 1871.

† *Ut sup.*, July, 1871.

‡ 'Memoirs Boston Soc. Nat. Hist.,' vol. i., 1867, "On the Spongiæ Ciliatæ as Infusoria Flagellata."

in our endeavours with the minuter individuals, which occasionally, we found, would allow a view through and through their entire bulk, and of course left full opportunity for a satisfactory study of the details of special parts, without our resorting to the dissecting needles. Anyone who knows, by experience, the intense contractility of the living sponge, can appreciate the advantage of not being obliged to destroy and sever parts of an organism from their natural relations. Premising thus, that everything has been studied in "place," even to the details of the monads, we shall endeavour to describe this sponge as if it were to be the type for future comparison.

General Plan.—The whole individual sponge is endowed with a double envelope (Fig. 1, a, a^1, c, d), the outer and inner parts of which are directly continuous into each other at many points. The outer division (a, a^1) lies at a considerable distance from the monadigerous mass (g), and is, as it were, suspended on the points of the larger, far-projecting spicules (e); just as a tent canvas is supported on the ends of poles. The inner division (c) closely embraces the monadigerous mass like an epidermis, and even plunges between the hollow groups of monads, forming to them a basis of support. The outer and inner divisions are continuous with each other at many points, as stated just now, but only where the larger spicules project. There the envelope (d) runs along the spicules, completely embracing them, as if in a sheath, from their tips to their bases, where they rest on the brown mass of monads. In brief, we might say that the sponge is covered with a miniature colonnade, whose ceiling is the outer division of the envelope, the pillars are the bundles of spicules, and the floor is tapestried by the inner division, which about the pillars hangs from the ceiling in lofty folds. The continuity of the outer division of the envelope is broken by numerous round or oval openings, of various and frequently changing sizes, sometimes very large, which allow a free ingress of the water to the space just beneath. These are the *afferent ostioles* (os), through and into which a constant current of floating particles may be seen moving with considerable vivacity. Here and there, scattered at wide distances, finger-like, hollow processes from the outer division arise singly and at various angles. Each is terminated by a large aperture, the *efferent ostiole*, from which a current of water and floating matter emerges with more or less spasmodic irregularity. The smaller individuals, from half a line to half an inch in diameter, possess only one such ostiole; and those an inch in diameter seldom have more than two or three like conduits; but they are very large, sometimes a quarter of an inch in length when fully extended, and of the proportions and taper of the human forefinger.

Plunging the focus of the objective to the floor of the colonnade,

the inner division (c) there is found to be pierced by much more numerous openings (i), but far smaller in diameter, and quite methodically arranged, each one corresponding to and overlying a hollow group of monads (h). The outer division is further embellished with irregularly-scattered minute spicules (e^1), which lie imbedded in the cytoblastema, parallel with the surface of the envelope, and occasionally crossing each other at various angles. To complete this general sketch, we will state more definitely the relation of the constituents of the monadigerous mass. There are essentially but two elements here; namely, the inner division (c) of the investing membrane, and the groups of monads (h) which are imbedded into it, below its surface. In a fully-expanded individual these groups seldom lie so closely as to touch each other. They vary considerably in size and are usually globular or spheroidal, and form a single *stratum*, with rather narrow interspaces (c^1) between them.

It seems proper here, at least for the sake of precision, that the *cytoblastematous basis*, in which the monad groups are imbedded, should be considered apart from the epithelium-like, inner (c) investing membrane which overlies it, although the two are essentially one; the epithelioid membrane, by prolonging itself between (at c^1) and beneath the groups, forming for them a continuous foundation. In this light, then, we shall speak of the monadigerous mass as consisting of three elements, namely, the inner investing membrane proper, the group of monads, and the *cytoblastematous basis*. This *basis* seems to constitute a large part of the bulk of the body, since it occupies all of the interior space beneath the monad groups. In specimens which grow over flat surfaces in depressed patches, or around stems of plants, it forms a relatively thin layer; but where the body stands out an irregularly-rounded mass, sometimes an inch in diameter, the cytoblastematous basis fills up the interior, in enormous proportion to the bulk of the monad layer.

Organography.

The Investing Membrane.—The *investing membrane* (Fig. 1, a, a^1, c, d) consists essentially of two histological elements, namely, a very diffuse *cytoblastema* (a^1) and irregularly-disposed cells (b, b^1, b^2) scattered through it. The intercellular *cytoblastema* forms a very thin layer (a^1) between the cells (b); but where the latter are imbedded in it, its outer and inner faces are as wide apart as the considerable depth of the cells demands; and thus it happens that the membrane (both the outer and the inner divisions) presents in profile (a^1, c, d) such an irregular thickness. The *cytoblastema* (a^1) is colourless, hyaline, and apparently homogeneous under a low power; but when magnified to about four hundred

diameters it displays a very finely granular aspect. It occupies wide intervals between the cells, certainly more than one-half, and fully three-fifths of the whole area of the membrane. Its apparent extent, in a general view, is even more than that, owing to the extreme transparency of the cells, and their consequent inconspicuousness. That the cytoblastema, notwithstanding its low undeveloped state, is the true contractile element in this membrane, there can scarcely be a doubt, when we consider both its widespread preponderance and its relative continuity, as contrasted with the scattered, disconnected condition of the cells (b^2) which are imbedded in it. Sometimes it is barely possible to discover even the trace of a cell on the border of an afferent ostiole (*os*), and in that case we must infer, inevitably, that it is cytoblastema which opens and closes the aperture. We find it, too, embracing the extreme tips of the larger spiculæ, where the cells utterly fail to appear.

The *cell element* (*b*) of this membrane is also in a lowly condition; only partially developed. There is no *cell wall*. What may appear to be a wall is really the thin stratum of cytoblastema (a^1) overlying the distal and proximal faces of the cell. This is our conclusion after the most critical scrutiny, with a carefully-corrected objective. Were it not, indeed, for the usually constant presence of a distinct nucleus (*n*) in each cell, we would be strongly inclined to look upon it as merely a dense collection of coarser granules than are generally diffused through the cytoblastemic layer. The irregular and jagged outline, and the caudate projections of the cells (b^2), also tend to tempt one to the latter view. The cell element in this case, then, corresponds only to what is usually considered the cell contents, and a nucleus. The contents are composed of coarse and fine grey granules, which at times are quite conspicuous, but most frequently are so transparent and slightly refractive as to appear, collectively, unless specially focussed upon, as a faint blotch in the investing membrane. This renders it all the more difficult to trace the outline of the cell, and particularly where it throws out irregular, caudate prolongations, to blend with those of other cells. We have been able to detect but one layer of cells in this membrane* when it is well stretched out. The depth of the cells, as may be seen in a sectional profile view (*b*), is about equal to their breadth, and their length is from one-half more than to twice their breadth; but frequently they are as broad as long. They stand in no particular relation to the ostioles; and, as stated above, sometimes scarcely touch their border. The *nucleus* (*n*) may be readily detected by its peculiar, strong refraction, and its considerable superiority in size over the granules. Its bright refractiveness in this connection reminded us of a contractile vesicle,

* Carter figures two or three cells overlying each other in *Spongilla alba*, 'Ann. Mag. Nat. Hist.,' July, 1857, Pl. 1, fig. 7.

but, although suspecting it of such a function, we could detect no change other than might be produced by the varying length and breadth of the cell, and the shifting of the relative position of the coarse granules. In the *inner division* (*c*) of the investing membrane the cells are usually smaller than those in the outer division, but differ in no respect otherwise, neither in form nor arrangement. They lie flat on their sides in the cytoblastemous layer; but, except in profile, they are most difficult to discover on account of the underlying brown mass of monad groups and granular interstitial substance.

Although we have been unable to discover any distinct cell elements in the cytoblastematous mass immediately around and beneath the monad groups, neither have we found it possible to distinguish it from the cytoblastema lying on the surface: and since the continuity between the two is unbroken, we must, perforce, consider them as one. The underlying portion of the cytoblastematous mass, however, is characterized by irregularly-scattered, moderately coarse, brown granules (c^1). These serve very well as a dark frame or setting to the monad chambers (*h*), and by contrast bring them out more strongly.

The Monad-cephalids.—We now proceed to describe the most essential feature of this animal, the *monads.* They are the characterizing, the dominating element, in reference to which the whole organism is contrived and constructed. They are not cells; they are the *heads* of a polycephalic individual, and consequently correspond functionally to the tentaculated heads of Polypi, and not to their interior epithelial cells. We must first describe what we call the monad chamber.

The *monad chambers* (Fig. 1, *h*; Fig. 2; Fig. 4) are deep spherical hollows which form the receptacles of the groups of monads (*j*). They are mere cavities, and have no lining wall.* They may be easily recognized, in young specimens, as clear, more or less circular, areas scattered in pretty close proximity to each other over the "cytoblastemic mass." Each chamber has a single, small, circular aperture (*i*) which perforates the inner (*c*) investing membrane, and allows egress into the circulatory apartment (*f*). The aperture (*i*) varies in size at times, and may even be completely closed. We have never seen it open wider than one-third the diameter of the chamber, and very rarely more than one-fifth as wide. That it is a true perforation, and not a clear spot, may be demonstrated by bringing a chamber into profile, so that its aperture (Fig. 4, *i*) lies

* The hollow groups of monads were originally described by Carter ('Ann. Mag. Nat. Hist.,' July, 1857) as lining an hypothecated vesicle, which he named the "ampullaceous sac." He has since ('Ann. Mag.,' Jan., 1859) revoked that view and adopted another. We believe him to be, excepting the inferred "ampullaceous sac." in the main, right in his first interpretation; but as our species are different we cannot speak definitely.

on the extreme border, and then an actual break in the continuity of the investing membrane becomes evident.

Entering this aperture we do not meet with any obstacle for a little distance around it; there is a clear open space (Fig. 4); but pressing onward beyond that, either to the right or the left or directly forward, the cavity appears filled by a collection of vibrating bodies. They seem to be arranged radiatingly from and about the centre. Close inspection, however, modifies this view, and it turns out that they are based upon the periphery of the chamber, and converge towards its centre, where is a small unoccupied space. We presently recognize these converging bodies to be *craspedote, flagellate* monads (*j*), so closely packed together, side by side, as to form a continuous *stratum* (Figs. 2 and 4) over the whole concave face of the chamber, excepting immediately about the aperture. Every feature of the monad is strongly marked; even the cylindrical collar is so heavy and conspicuous that its outlines may be seen with as low a power as two hundred diameters. We have studied these bodies with a $\frac{1}{8}$th-inch objective, and found it not at all difficult to focus down upon the details of their organization, without pressing upon or even touching the specimen.

These monads are in every general essential identical with those which we originally found in *Leucoselenia*, and like those also, recently described by Carter,* in *Grantia compressa.* They are attached to the concave face of the chamber by their posterior end (Fig. 4, *j*); and the anterior extremity, with its flagellum (Fig. 3, *l*) and collar (*k*), projects freely into the open space, and toward the centre of the apartment. When fully expanded, the length of the body and collar together is about one-third, or a little more, of the diameter of the chamber; so that nearly one-third of the latter is unoccupied at the centre, except by the tips of the flagella converging from every direction. As the monads lie touching each other on every side (Fig. 2), they mutually flatten their bodies, sometimes so much as to give them a strong polygonal outline; or, when the whole mass is expanded, they scarcely impress each other, and therefore retain a rounded contour. By plunging the focus so as to look into the aperture of a chamber, down upon the monads at the bottom (Fig. 2) of it, an end view of each cephalid is obtained. From this point the foreshortened cylindrical collar looks like a strong, dark circle (Fig. 3 *a*, *k*), which retains its conspicuousness as we plunge down farther, even to the base, where it is attached to the body (*j*). The outline of the latter is considerably without the "dark circle," the two being concentric to each other. At the same time we see in the centre of the dark circle a black spot (*l*) which may also be focussed up and down upon, and hence it is inferred to be a continuous line foreshortened. Other views (Fig. 3, *l*) con-

* 'Ann. Mag. Nat. Hist.,' July, 1871.

firm this, and show that it is a single *flagellum.* The monads are so transparent, and the organization so distinct, that the collar and flagellum may be seen clearly from an opposite point of view, looking directly through the body of the cephalid. This, too, is the best position from which to study the *contractile vesicles.*

A sectional profile view of a group (Fig. 4), to be obtained by plunging the focus half-way through a chamber, serves best to disclose the manner in which the posterior ends (*j*) of the monads are affixed to the concave face of their receptacle; and we also here obtain a strictly profile aspect of a monad. Figure 3 is such a view, representing a single cephalid, under a much higher power than in Fig. 2 or 4. An excellent and least obstructed side view, but not strictly a profile, is to be had by focussing upon the monads immediately about the aperture of the chamber. Here we look directly into the door-way, or through the bordering, transparent epithelioid membrane which it penetrates.

The body proper (Fig. 3, *j*) of a cephalid is a little shorter than it is broad; on the whole, spheroidal in shape. Its posterior end is broadly rounded, and so is its anterior extremity. In front rises a cylindrical, membranous "collar" (*k*), which tapers slightly and projects forward to a distance equal to considerably more than twice the length of the body. Its diameter is not more than two-thirds, or even less than that of the body. Although colourless and homogeneous, it is remarkably conspicuous on account of the thickness of the membrane of which it is composed. Near its open extremity it is more transparent and less obvious than toward its basal attachment.

The *flagellum* (*l*) arises from the centre of the anterior end of the body, in the midst of the area which is surrounded by the membranous cylinder (*k*), and without tapering extends a little farther than the open end of the latter. It vibrates usually throughout its length, but is most active near its tip. We have never seen it assume a rigid, arcuate position, as in some other species of monads. It is particularly remarkable for its want of transparency, and looks like a black thread more than any vibrating cilium that we have ever met with. Its action, at times, is rather that of a strong wriggle than a vibration.

The *contractile vesicles* (*v*).—The body of the monad is distinctly marked by a coarse, scattered, brown granulation, with two or three rather large, clear spots, at a considerable distance from each other, but always close to the periphery. These clear areas are the *contractile vesicles* (*v*). They do not occupy any particular place in the body, although they usually are not in front. The systole and diastole are extremely slow, but very distinct, if sufficient patience is summoned to watch them fixedly, and without interruption. The last third of the systole is abrupt, and then only

does the vesicle appear to contract suddenly; whereas by watching it through a complete circuit of diastole and systole, one learns that its function is, on the whole, performed very slowly. This very abrupt movement, quite happily, may serve to rebut any such objection as that the otherwise tardy action is merely the result of protoplasmic contraction of the body, as in certain Palmellate Zoospores. Their immovable position, as regards the body contents, is another item of rebutting evidence.

The *Spiculæ* (Fig. 1, *e*, e^1) are very slender, slightly curved, needle-shaped bodies, gradually tapering to a sharp point at each end. They have a bright amber colour, and a rather dark, strongly refractive outline. From tip to tip they are slightly roughened by irregularly-scattered, low, but acute prominences or knobs. There are two kinds of spicules, large and small, but they differ in no other respect. The larger (*e*) are from four to six times longer and thicker than the smaller ones. They occur in bundles of two, three, or four; and act as props to hold up the outer investing membrane, as described in the early part of this article. They seldom arise perpendicularly from the monadigerous mass, but more or less obliquely; and, in forming bundles, stand across each other like stacked arms. We seldom found spicules penetrating the monadigerous mass far beyond the epithelioid, inner investing membrane. They evidently belong, universally, to the investing membrane, and assist it in forming a framework in which the inner mass is suspended. The *smaller spicules* (e^1) are strictly confined to the outer division (*a*) of the investing membrane, and lie there on their sides, completely immersed in its thickness. They are scattered irregularly and sparsely about, and frequently cross each other at varying angles. We observe no nearer approach to a methodical arrangement among either the large or the small spicules; yet their very irregularity, being after a kind, and constant in that kind, may be recognized in some sense as methodical.

General Considerations.—Seeing the secluded position of the monad-cephalids, deeply ensconced in little chambers below the general surface of the circulatory apartment, it is not directly evident that their *flagella* have any agency in keeping up the inflow and outflow of currents through the afferent and efferent ostioles. Nowhere else are vibrating or non-vibrating cilia or cilia-like bodies to be met with than in the monad chambers. And since the efferent ostioles are irregularly interspersed among the much more numerous afferent ostioles, we cannot conceive how the flagella in any way could influence currents to move in a particular direction, from the smaller apertures toward the larger ones. They no doubt keep up a direct flow of matter into the sunken chambers, but the current comes from the inner depths of the circulatory apartment, and far away from the ostioles. In this

way only a turbulence of floating matter is sustained, but the general, great current is due to a far different cause. We conceive that the contraction and expansion of the body-mass in general, modified by the alternate opening and closing of the afferent and efferent ostioles, is the true motive power in this phenomenon. We have observed, often, that the outer division of the investing membrane is not kept at a uniform distance from the central, monadigerous mass; at one place it will be found to be close to its inner division, so that the circulatory apartment is very shallow there, while at another point the two divisions of the membrane are widely separated, and the circulatory apartment is very deep; and between the shallow and the deep apartments a curtain is drawn, more or less completely, extending from one pillar-like bundle of spicules to another. Each of these temporarily enclosed portions of the general apartment, it is plain now (although our actual observation on this point is very defective), may contract or expand without disturbing the contents of any other. Such an apartment with its afferent ostioles closed, may be contracting and forcing a current out at its efferent ostiole, while a neighbouring apartment may have its efferent ostiole closed, and expanding, draw in current through its open afferent ostioles.

We regret that we have not the means, in this locality, for completing these researches. Our specimens were gathered, and studied on the spot where they lived, in the western part of Massachusetts, several hundred miles away from our present residence. Unfortunately we put off the attempt to feed the sponge with coloured matter until we had completed other methods of investigation, and then we were prevented by circumstances from carrying out our designs.

In regard to the afferent and efferent canals, seen by Carter * in the monadigerous mass ("*parenchyma*" Carter), we have not met with any trace of them in the species described in this article. It is possible that they may exist in the oldest and largest individuals, but as we worked only on very small and transparent specimens, our direct observations, in this respect, strictly apply to the latter. It is more likely that ours is a different genus from the *Spongilla* of Carter, in favour of which we cite the curious fact that each aperture, in the inner division (not mentioned by Carter) of the investing membrane, exactly overlies and is inseparable from the entrance to a monad chamber ("*ampullaceous sac*"; *partim*, Carter); so that whatever enters these chambers must go out by the same way that it came in; not out into a system of branching canals, burrowed in the monadigerous mass, but into the great circulatory apartment.—*Silliman's American Journal*, Dec., 1870.

* 'Ann. Mag. Nat. Hist.,' 1857, *ut sup.*

V.—*The Refractive Powers of Peculiar Objectives.*

By Robert B. Tolles (U.S.).

In my brief note of December last (1871), communicated to your Journal, I described the action of an immersion objective of 100°, constituted of a dry objective of that angle at "uncovered" and of additionally two hemispherical lenses in front, and having the objects in liquid or in cement between. I now propose to borrow in part Mr. Wenham's diagram,* for further illustration. This diagram, Mr. Wenham says, "shows the marginal rays of mean refraction projected in the exact position that they occupy through the $\frac{1}{8}$th described." The extreme aperture is 130°.

Fig. 1 annexed, *except the dotted lines*, is a copy of Mr. Wenham's as to the front lens and projected rays, with the addition of a double-hemispherical front system, combining the functions of both the immersion front and the balsam-mounted object-slide. It will be noticed that the rays of the $\frac{1}{8}$th objective, as traced by Mr. Wenham, meet at the centre of this interposed *sphere*, because their paths are coincident with its radii. For the same reason this now immersion $\frac{1}{8}$th must be of the same angle as the dry $\frac{1}{8}$th described by him, *viz.* (about) 130°. Or to state the case in the words of Mr. Wenham, expository of a surely coincident case, the object being in balsam, "then of course it" (the object) "can be illuminated from all angles, and seen by the full aperture of an object-glass."†

So far there is evident agreement. But the illustration can teach more than this. With balsam between the hemispheres, as before, the object will be traversed and illuminated by a pencil of nearly maximum angle.

The whole pencil may or may not be passed through the objective used *according to its construction*. But if air be between the hemispheres and the plane surfaces dusted with minute objects, does anyone imagine large angular pencil to reach the object in this case? Either from above or below? Certainly not anyone. The objective being thus used, *dry*, Mr. Wenham's defiance‡ might be very safely tendered.

When air is between the hemispheres, the immersion $\frac{1}{8}$th is limited in its angle to the capacity of the dry $\frac{1}{8}$th in., *when that is used for balsam-mounted objects*. That is to say, the first interior reflecting surface (air outside) that the rays meet in any case limits the angle to 82° (–), according to the law of total reflexion at interior surfaces. But with any fluid of higher refraction than air between the hemispheres, of course the interior angle of the pencil

* Found at p. 19, No. xxv., 'Mon Mic. Jour.'
† See 'Mon. Mic. Jour.,' No. xxxvi., p. 292. ‡ Ibid., No. xxvii., p. 118.

transmitted will be increased, and for this case Mr. Wenham's challenge cannot stand.

For the sake of the simplest, clearest exposition I have in design as related made my immersion objective for illustration of four systems, borrowing Mr. Wenham's dry $\frac{1}{8}$th diagram in part as before stated. It only now remains to point out the variation necessary to construct the immersion objective of three systems and of approximate, interior angles.

For this purpose I will adhere to the same $\frac{1}{8}$th in. diagram, and indicate the modification in front lens needed to accomplish the result.

In the Fig. 2, the outside dotted line indicates the convex surface of the dry front as drawn by Mr. Wenham; the continuous line of a shorter radius within that is to indicate the curvature necessary to produce a $\frac{1}{8}$th-inch immersion of angle about 105° interior pencil. What is intended is to increase the refraction of the convex surface of the front by sharper convexity, or higher refractive material, or *both*, to the extent necessary to make up for the diminution at the plane surface which takes place according to the refractive power of the medium flowed in. "The mere fusing by a refractive medium of the thinner or immersion lens and cover into one," as Mr. Wenham says; but here with the difference that the front lens remains of the same thickness as the dry, while the convex surface has (according to requirement of the case) increased refraction.

Below the new front of this now immersion $\frac{1}{8}$th is represented the thin cover and thin slide (increased about four times with, understood to be, balsam-mounted objects between. The ray *r*, Fig. 2, may be continued—tracing the ray from above in this case —to the object under the cover, because, although in the case of water there is some positive refraction at the plane surface of the front, it is exactly, or very nearly, balanced by negative refraction of the first surface of the covering glass, *viz.* tracing the ray from above, downward towards the object, as Mr. Wenham does in his treatment of the subject in his diagram. Moreover, if the flowed-in medium were of the same refractive index as the glass, the line would be straight. Well, in continuing this ray of $52\frac{1}{2}$° incidence to the lower surface of the slide, the law of total reflexion (interior) comes into effect. Of course only 41° incidence, and 82° angular pencil, will have passage through the lower surface of the slide. This total reflexion boundary forms an absolute limit of angle for objects in balsam. I might refer my critic to my first communication* for the proper method to adopt to gain a larger pencil, but otherwise and easier let reference be made to Fig. 3 annexed, intended to represent an ordinary object-slide with covering glass, and

* 'Mon. Mic. Jour.,' No. xxxi., p. 36.

FIG. 1

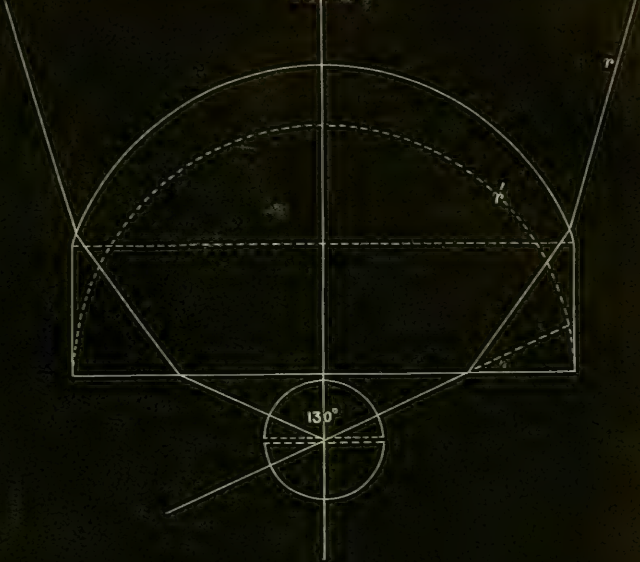


FIG. 2

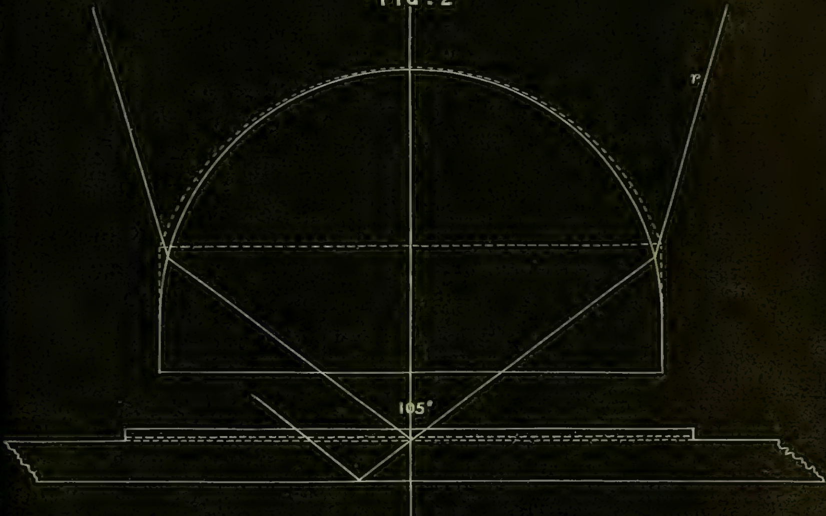


FIG. 3

object in balsam. The ray *r*, identical with that of Fig. 2, having passed through the liquid above the cover, and the covering glass itself, will, or a ray correspondent to it, proceed to the convex surface of the cemented plano-convex lens below the slide, and emerge there as traced in the figure, or substantially so. An arrangement for procurement of 130° is indicated in Fig. 1, where the dotted lines show a sharper curvature as also thinner lens. When this lens is moved towards the system next back of it until the surface meets the ray *r*, coincidently with the dotted line *r'*, then that ray can have refraction correspondently with the line drawn by Mr. Wenham for 130° in his dry $\frac{1}{8}$th diagram. This can be strictly so when the medium between the plane surface of the front and covering glass is of the same refractive index as the glass. In this case, too, no limitation by total reflexion. But if water is used, there would be limitation, and within 130°. To gain fully that aperture, a medium of somewhat higher refractive index than water would be needed. But only the refractive ability of the connecting medium is the real practical limit of angle of transmitted pencil in the immersion objective below maximum angular aperture.

The limitation takes place at the upper surface of the covering glass, not *necessarily* anywhere else, with balsam-mounted objects. If air is the medium in the interspace above cover, only 82° approximately can be obtained, with whatever appliances.

As stated in my first communication, an immersion objective, if used below the slide as condenser, or any form of immersion condenser of *sufficient angle*, has the same action (of course) as the plano-convex lens, to increase the angle of the interior pencil, according to the immersing medium used.

Mr. Wenham, in his last note,* asks some questions, as follow:— "Does he" (the writer of this) "really expect us to believe that he can obtain all his aperture by the back combinations alone?" and in response I have described a means of procuring 130° with *three* "back combinations" in use instead of two.

Again, "or that any of his objectives, when duly adjusted for an immersed object, and thus showing a large aperture when measured in air, will retain the same angle when the front is immersed in water?"! *Certainly not* (*vide* my own experiment described in the first dozen lines I ever wrote on the subject, 'Mon. Mic. Jour.,' No. xxxi., p. 37, where an objective of 170°, measured in air, is credited with only 100° when immersed in water). By the way, Mr. Wenham in his own experiment† reaches the same result, *viz.* "a $\frac{1}{15}$th of 170°" in air, *in water* showed an "apparent aperture" of 100°. The objective being built for water-immersion use (doubtless) that was the proper medium for the

* 'Mon. Mic. Jour.,' No. xxxvi., p. 292. † Ibid., No. xxxii., p. 86.

pencil measured to traverse. No, not the same in water as in air necessarily; 100° of angle is appreciably more than 82°, and that difference is the question at issue.

Lastly, Mr. Wenham says, "His position is an impossible one. In a corrected high-power object-glass, when immersed, the focus does not fall in the centre of the hemisphere, but is considerably beyond it."

Very well; that does not affect my position. He will not deny that it *may* fall in the centre of the hemisphere and give view of the object.

In the case that I offered in my first communication, also as just related that one given by him in the next following number of this Journal, an objective of 170° in air, gave only 100° in water. But that is enough for my "position," and 100° allows of projection "considerably beyond the centre of the hemisphere" for its focal point. The position seems a very possible one, and Mr. Wenham seems to furnish proof of its validity.

Boston, *January 19th*, 1872.

VI.—*A Few Additional Remarks on "The Examination of Nobert's Nineteenth Band."*

By F. A. P. Barnard, Columbia College, New York.

The pages of a scientific magazine are too valuable to justify the intrusion upon them of matters of merely personal interest. But when a person is placed before the readers of such a magazine in a light in which he does not appear to advantage, he has no alternative but to ask for a brief hearing.

Early in the year 1868 I made a long-continued and laborious examination of Nobert's Nineteen Band Test-plate. I saw lines in the nineteenth band which I supposed to be the real ones. I made counts of these lines with a filar micrometer; and from these counts deduced by calculation the number of divisions like those counted which would correspond to an English inch.

It was only with the extremest difficulty that I could obtain what seemed to be the resolution. To secure the necessary conditions of illumination and focussing cost me often much time; and often I failed, after very protracted trial, to secure them at all. Moreover, when they were obtained, the image was so feebly illuminated as to require an extremely acute vision (which I think I possess) to see it at all; and the labour of observation was so severely trying to the eyes, that I should not be willing to go through such a course again.

Because of these circumstances, the results were not satisfactory to me. I therefore did not publish them, nor even speak of them beyond a limited circle of my acquaintances. I even went so far as to ignore them altogether when speaking on the subject in public; not regarding them as sufficiently important to justify me in qualifying the expression of opinion quoted by Col. Woodward in the January number of this Journal, that the lines of the nineteenth band had "never been fairly resolved."

My results and my method of reaching them were communicated to Mr. Stodder in response to a request made directly to me by him. He published them, and they became subject of criticism by Col. Woodward. Had Col. Woodward confined his criticism to the results, I should have had nothing to say; but inasmuch as he attacked the principle on which I proceeded, I felt bound to vindicate the principle as one which is unquestionably sound. I could not vindicate the principle without referring to the observations for illustration. If in doing this I have seemed to "reconsider" my opinion formerly expressed as to the value of those observations, it is simply because he has made it a logical necessity for me to do so. I certainly saw *something* and counted something. No subsequent expression of opinion of mine can alter that fact. And in order that the readers of the Journal might be in possession of the whole case, I should wish to place before them all the original minutes of the observations, if it were in my power. They have been mislaid, but I think not lost; and I hope to publish them hereafter. In searching for them I have encountered a series of observations made in the following year, when the dry-working front of the objective had been replaced by an immersion front by Tolles. The following examples, copied literally from the notes, will illustrate the mode of proceeding. In the first four bands the entire breadth of the band is measured: in the nineteenth, the measurement extends to only twenty divisions. The second number of each equation is the equivalent number of the micrometer.

1st Band.	$\frac{6}{1000}$ line = 1161 div.	6 lines = 1,161,000 div.
		1 line = 193,500 div.
2nd Band.	$\frac{9}{1500}$ line = 1159 div.	9 lines = 1,738,500 div.
		1 line = 193,167 div.
3rd Band.	$\frac{12}{2000}$ line = 1158 div.	12 lines = 2,316,000 div.
		1 line = 193,000 div.
4th Band.	$\frac{14}{2500}$ line = 1076 div.	14 lines = 2,690,000 div.
		1 line = 192,143 div.
19th Band.	$\frac{20}{10000}$ line = 391 div.	2 lines = 391,000 div.
		1 line = 195,500 div.

Objective, $\frac{1}{12}$th immersion.

If the rulings and the counts are both correct, one division of the first band should be equal to ten of the nineteenth; one of the second to $6\frac{2}{3}$ of the nineteenth; one of the third to five of the nineteenth; and one of the fourth to four of the nineteenth.

$$\text{I.}\quad \frac{1161}{6} = 193{\cdot}50. \qquad \text{And } \frac{391}{20} \times 10 = 195{\cdot}50.$$

$$\text{II.}\quad \frac{1159}{9} = 128{\cdot}78. \qquad \text{And } \frac{391}{20} \times 6\tfrac{2}{3} = 130{\cdot}33.$$

$$\text{III.}\quad \frac{1158}{12} = 96{\cdot}50. \qquad \text{And } \frac{391}{20} \times 5 = 97{\cdot}75.$$

$$\text{IV.}\quad \frac{1076}{14} = 76{\cdot}86. \qquad \text{And } \frac{391}{20} \times 4 = 78{\cdot}20.$$

The coincidences are not exact; but they are too close to be accounted for on the supposition that the lines in the nineteenth band are spurious.

The following are specimens of measurements made at the same time to determine the value of a French line in divisions of the filar micrometer, by measuring the divisions of a stage micrometer, in which the British inch is divided into 1000 parts; a covering glass having first been placed over the stage micrometer, as nearly as possible equal in thickness to the Nobert plate. Both lines of the filar micrometer were employed in this measurement, having been first adjusted so as to throw the total index error upon one. Each thread was then moved over one one-thousandth of an inch, with the following results:—

Index error, left screw $= 0$, right screw $= +42$.

I. Left Screw. Right Screw.

$$\frac{1}{1000}'' = 2162 \text{ div.} \qquad \frac{1}{1000}'' = 2211 - 42 = 2169 \text{ div.}$$

$$\frac{1}{1000}'' = 2162 + 2169 = 4321 \text{ div.}$$

1 inch = 2,160,500 div. 1 line (British) = 180,041·7 div.

$$180{,}041{\cdot}7 \times \frac{81}{76} = 190{,}676 \text{ div.} = 1 \text{ line French}$$

Another portion of the stage micrometer being brought into the field, a second measurement was taken, thus:—

II. Left Screw. Right Screw.

$$\frac{1}{1000}'' = 2167 \text{ div.} \qquad \frac{1}{1000}'' = 2248 - 42 = 2206 \text{ div.}$$

$$\frac{2}{1000}'' = 2167 + 2206 = 4373 \text{ div.}$$

1 inch = 2,186,500 div. 1 line (British) = 182,208·3 div.

$$182{,}208{\cdot}3 \times \frac{81}{76} = 193{,}011 \text{ div.} = 1 \text{ line French.}$$

The stage micrometer is probably not greatly to be relied upon, and the covering glass may slightly differ in thickness from that of the Nobert plate. It is also coarsely ruled, but considering these circumstances, these last determinations accord fairly with those deduced from the Nobert plate previously.

In conclusion, I wish to say that I claim no merit for the resolution of the fine lines of Nobert's nineteenth band, and never have claimed any; what I claim, and all that I claimed in my former communication is, that the method employed by me for testing the character of the lines observed is, in a scientific point of view, entirely trustworthy.

PROGRESS OF MICROSCOPICAL SCIENCE.

Bilharzia hæmatobia (Cobbold) in a Manchester Patient.—Dr. Henry Simpson gave the Manchester Philosophical Society an account last year of a case of his in which this rare worm had appeared. Through inflammatory changes leading to loss of substance, the ova become free in the bladder, and are washed away in the urinary secretion in immense numbers, along with blood disks and pus-corpuscles. They are generally ovate in form, but vary somewhat in outline. At one end the shell is produced into a short spike, something like that on Von Moltke's helmet. Occasionally this is placed laterally. They vary in length from $\frac{1}{170}$th to $\frac{1}{200}$th of an inch, and in breadth from $\frac{1}{400}$th to $\frac{1}{500}$th of an inch. The shell is without any distinct operculum. The contents of the egg are seen in all stages of development, from scarcely distinguishable granules, enclosed in a vitelline membrane, to the perfectly-formed ciliated embryo, exhibiting active movements of the body and rapid play of the cilia while still enclosed in the shell. Sometimes the head of the embryo lies towards the spiked end, sometimes to the plain end of the shell. Free, living embryos are often met with in the urine, and it is curious to watch the mode in which they escape from the shell. The general shape of the embryo is elliptical; they are abundantly supplied with cilia, especially at the anterior extremity, and show distinct traces of a water vascular system. The development of this entozoon in all probability follows the same general plan as that of the other Trematode worms, or Flukes, which pass through several phases or alternations of generation; one or two intermediate hosts, generally mollusca or aquatic larvæ, being necessary before the adult fluke becomes parasitic in the body of the vertebrate animal destined to be its host.

Mode in which Cliona burrows.—Mr. Edward Parfitt publishes a paper on this subject, and the boring of mollusks. He differs from Dr. Hancock, who says, in regard to *Cliona*, "that no other excavation, whether of worm or mollusk, presents a surface anything like that of the burrows of sponges; and were no other proof at hand, this puncturing would be sufficient to establish the fact that they possess the power of enlarging their habitations." Now, says the author, if Mr. Hancock's assertion be right, these silicious tools ought to leave their marks on the walls of the holes accordingly. We should have either lines corresponding with the direction of the burrows or transversely, or both, according to the contracting of the investing membrane; but so far as he has investigated this subject, he has not been able to detect any scratches of this kind. In fresh-bored cavities into solid shell, the walls are lined with the thin, pale, yellowish process of the sponge, composed of the investing membrane and the inner sarcode, on which was scattered a few sponge cells. In these membranes are stuck, almost at random, a few spicules, the principal use of which is to hang on the investing membrane, and it is on these that the sarcode

proceeds or creeps along; but so far as he can discover, they do not assist the sponge in excavating in any way whatever, only that they are the means by which the sarcode advances. Now it seems to him that wherever the sarcode, or rather the investing membrane, touches the shell, the latter shows an eroded surface; the minute points and pits can easily be seen with a quarter-inch objective. If one of those fresh-bored holes be examined horizontally in good light, and even with an inch objective, it will be seen that these holes are not smooth, although they appear so to the unassisted eye, and even with a good pocket lens; but when carefully examined, a number of points and angular processes are seen standing out into the hole in various directions, showing at once that the hole had not been cut with anything moved with a rotatory motion. He carefully examined these holes all the way down to their apex, and he finds the same appearance all the way; similar projections of shell are seen all the way down. The apices of these holes are not round, but are more or less angular. Many of them have a triangular impression, and all of them show the minutely-roughened surface, the minute points, and pits of an eroded surface. Now he has carefully examined these sponges for some erosive agents, the same as did Mr. Hancock; but he has, with that gentleman, not been able to detect any; yet in his own mind he feels certain that it is by the action of some agent of this kind that these holes are eroded both by the annelids and sponges. Our not finding an acid in these animals does not prove that they do not use, or separate, or secrete, an acid or an erosive agent; and he sees no reason whatever for doubting, neither does he see any difficulty in believing, in their decomposing the sea water, or separating carbonic acid from it, and using it as soon as separated. By this means it is easily conceived how these animals work, and it is also seen why we could not discover the agent, for they kept none in stock, but manufactured it as they wanted it. He illustrates his views by a very well-drawn plate, showing the magnified borings.

Unfortunately, however, this subject has recently had devoted to it a paper,* by Mr. J. G. Waller, which is as full of exact information and careful deduction as is possible. Mr. Waller, unfortunately for Mr. Parfitt, proves beyond contradiction that this is really not a burrowing sponge at all; and his paper, though too long for insertion here, will be read by those interested in the subject with advantage. His arguments against the mechanical powers of the sponge seem to us to be quite sufficient. In the section of an oyster-shell, of which he gives a figure, we can see the burrows filled with a sponge, and at its distant extremity, where is the latest growth of the sponge, we find the membrane pellucid, with but few spicula. Going back into the older portions, this membrane gets gradually more full of colour, denser in character, the spicula increasing in number, until they become almost matted together. We then come to that part on the edge of the shell where the laminæ are wide and open, whose spaces, beside the burrows and connecting with them, the sponge has occupied, and here has communication with the outer world. Hitherto the dermal membrane

* Read before the Quekett Club, and published in its Journal, January, 1872.

has been protected by the shell it covers; now, as this is wanting, we find the means it takes for that purpose singularly effective. The spicula in all other parts of the sponge have been irregularly disposed on the membranes, which is a mark of the genus to which Dr. Bowerbank has given the characteristic name of *Hymeniacidon.* But here they form themselves into a regular wall, closely packed together, parallel to each other, so as to present a formidable array of pikes to any adventurous intruder who would attack the domicile. The sarcode here thickly overlays and invests it. Here are found the pores—the inhalent organs—not, however, very conspicuous, but sometimes distinctly seen with an inch lens. So that, instead of finding the sponge in possession of an apparatus of attack and destruction, which has been assigned to it, we see that its offensive powers, if we may so call them, have been concentrated into a system of defence, to screen it from assaults from without; and that no part of the sponge has so powerful an organization as this prepared for its protection.

Microscopic Structure of the Pitchstones of Arran.—This is the title of an instructive paper, with a plate, in the 'Geological Magazine' (January). We do not ourselves think that the specimens represent anything but mineral formations, though of course we cannot very well say, not having seen the specimens. The author (Mr. S. Allport) says that a thin section, examined under the microscope with a one-inch objective, is seen to consist of an amorphous glassy base, containing numerous long, slender prisms of a green pyroxenic mineral; the latter are occasionally isolated, but generally form the axes to which are attached innumerable minute pale green crystals, arranged in exquisitely beautiful groups; some wonderfully like fronds of ferns, others bearing the closest resemblance both in form and colour to some of the microscopic fresh-water algæ; in fact, the field of view appears to be crowded with minute ferns, and the most elegant sprays and tufts of *Batrachospermum.* The glassy base has a pale yellowish tint in the open spaces between the groups; but under a higher power the colouring matter is resolved into a mass of translucent granules and minute crystals, the latter being much smaller than the belonites which form the fern-like groups. A comparison of many specimens, affording gradations in size, shows clearly that all these crystalline particles consist of the same pyroxenic mineral as the larger prisms which form the axes. It is worthy of remark that the tufts and aggregations of belonites are invariably surrounded by a border of clear colourless glass. Placed between crossed prisms, the section is, if possible, still more beautiful; the tufts and sprays then appear to be formed of bright gold powder on a perfectly black ground, with fine brilliant dust scattered in the dark spaces between the groups.

The Gregarinidæ developed from Amœbæ.—The 'Lancet,' in a recent number, has taken up the subject of M. E. Van Beneden's paper, of which a short notice was given by us some time ago. It says that the author has followed, in the case of the *Gregarina gigantea*, the entire series of metamorphoses undergone by the masses of protoplasm proceeding from the psorospermia into a fully-developed

gregarina measuring two-thirds of an inch in diameter. He found in the intestine of the lobster small masses of protoplasm destitute of nuclei, finely granulated, and performing continuously ordinary amœboid movements. These bodies he compares to the *Protamœba primitiva* or *Protamœba agilis* of Haeckel, and classifies them with the true Gymnocytodes of that author. Bodies of precisely similar appearance are also found, however, which are incapable both of locomotion and of change of external form. In these a homogeneous marginal zone and a granulated internal mass come to be distinguished. The gymnocytode invests itself with a membrane, and is thus converted into a leptocytode (of Haeckel). Each cytode now thrusts out two arms, which are prolongations of its substance, and each of which is destined to become a gregarina. The first one that makes its appearance is the largest, and is characterized by its marked granulation and great mobility. It detaches itself from the cytode and becomes free. Before this occurs, however, the smaller arm is thrust forth, which is not granular, and is motionless. After the first or mobile arm has separated from the cytode, the second arm becomes as mobile as the first was, and absorbs the whole body of the cytode into itself, just as the embryo of a mammal absorbs the contents of the yelk-sac. The two protoplasmic arms thus produced from a single cytode—Pseudofilaria, as Van Beneden terms them—move with extraordinary vivacity in the intestine of their host. After the lapse of a certain time the movements become less lively; the pseudofilaria become shorter and thicker; nucleoli, and subsequently a nucleus, make their appearance in their interior. At a still later period a homogeneous cortical layer (membrane) is differentiated from the granulated contents, at one pole of which strongly refractile granules collect, and the whole organism now appears as a well-defined small gregarina, which soon augments in size.

Laboratory for Marine Zoology.—Dr. Anton Dohrn, in a letter to Professor Agassiz, who has communicated it for publication in the 'American Naturalist' (January, 1872), writes that he has matured a plan which has for many years been in the minds of many zoologists; that of establishing a large laboratory for marine zoology in the Mediterranean. He has obtained permission of the authorities of the city of Naples to construct a large building at his own expense, in the Villa Reale at Naples close to the sea, containing a large aquarium for the public, and extensive rooms for naturalists of every country. Dr. Dohrn, with two or three other German zoologists, will settle there and conduct the administration of both the aquarium and the laboratories. He wishes that information regarding this proposed laboratory be widely extended, and earnestly invites all who may go to Naples to visit the aquarium. An annual report of the work done and progress made at the zoological station will be published. A committee has already been formed to give further dignity and importance to the project, consisting of Messrs. Hemholtz, Dubois-Reymond, Huxley, Darwin, Haeckel, Leuckart, Van Beneden, &c., and in America Professor Agassiz.

Is Guano Bird-dung or Infusorial Deposits.—This question has recently attracted a good deal of attention from several naturalists, especially from Professor A. M. Edwards, of the Lyceum of Natural History of New York. Professor Edwards says, that in California there is a deposit of "Infusoria" improperly so-called, accompanied by bitumen, which bitumen the gentlemen of the State Survey believe has been derived from those "Infusoria," and that contiguous thereto we have guano deposits. At Payta, in Peru, Dr. C. F. Winslow discovered an "Infusorial" deposit, almost identical in character with the Californian one: near by are bitumen springs, and lying off the coast are the guano islands of Lobos, Chincha, Guanape, and others. At Natanai, Japan, we have extensive "Infusorial" strata and bitumen; it is not recorded whether guano occurs in that quarter. In the island of Barbadoes we have "Infusorial" strata, bitumen, and near by the guano islands of the Caribbean Sea; and he is informed guano is abundant on the small islands and rocks nearly throughout the West Indian Archipelago. In the island of Trinidad we have "Infusorial" strata and bitumen, and of course adjacent guano. At all of these localities volcanic action is evident, but we have some localities of guano without "Infusorial" strata or bitumen as yet recorded, while we have the celebrated "Infusorial" strata of Virginia, which by a little stretch of the imagination may be supposed to be related in some way to the petroleum of West Virginia and Pennsylvania. In Algeria we have "Infusorial" strata and bitumen, but he never heard of guano having been found near by. However, now that attention is called to this fact, it is to be hoped that more careful observations will be made connected with the subject, and he hereby calls on all sciencists and travellers to do all they can to assist in the elucidation of this interesting and important matter. From all of these facts, and others that he has collected of no less importance, derived from chemical and microscopical characters, he has come to the conclusion that guano is not the excreta of birds deposited upon the islands and mainland after its upheaval, but that it is the result of the accumulation of the bodies of animals and plants, for the most part minute and belonging to the group which Haeckel has included in a new kingdom, separate from the animal as well as the vegetable, under the name of Protista, and subsequently upheaved from the bottom of the ocean. Subsequent chemical changes have transformed it into guano, or heat and pressure have so acted upon it that the organic matter has been transformed into bitumen, while the mineral constituents are preserved in the beautiful *atomies* that make up the mass of the extensive "Infusorial" strata found in various parts of the world.

The Natural History of the Passifloraceæ.—Dr. Maxwell T. Masters, F.R.S., who is now almost our only British physiological botanist, has given the world a most valuable essay* upon the whole group of Passifloraceæ. In this he has devoted a special portion to the subject of the minute anatomy of the group, which is of great interest. Dr. Masters has not followed it out as closely as he might have done, but still his

* 4to, and two plates.

work is very good, and deserves the attention of microscopists. With regard to the sepals of this order he says that their lower or outer surface are covered by an epidermis, the cells of which are somewhat cuboidal, but sinuous in outline above, and thickened on their upper wall. They are interrupted here and there by oval stomata, bounded by two sausage-shaped guard cells containing chlorophyll, a substance not met with in most of the other epidermal cells. The inner or upper epiderm of the sepals is of a similar character, and is likewise perforated by stomata. Some of these stomata are of an imperfect character, in that the separation into two guard cells does not take place. The form of the aperture remains unaffected; but it is bounded by a single cell instead of by two. According to Von Mohl, the stomata originate from the subdivision of a single cell, each of the subdivisions becoming a guard cell. In the imperfect stoma above described there has been arrest of development, as it were, and the sphincter of the stoma is formed of one cell instead of by a pair. Charles Morren,* who was the first to notice this condition of the stomata, mentions the existence in *Passiflora quadrangularis* of still more imperfect stomata on the calyx, consisting merely of one semi-lunar guard cell, as if the fellow cell were suppressed. Morren states that he saw, in one case, an aperture, as of an ordinary stoma, between the side of this solitary sphincter cell and the adjoining epidermal cell. He has not himself seen these half stomata, probably because his observations have not been sufficiently extended. Beneath the outer epidermis are three or four rows of spheroidal cells containing chlorophyll. Within this (in *P. alata*) there is a quantity of loose spongy cellular tissue, consisting of irregularly branching cells, between whose subdivisions are left large spaces or *lacunæ*. These cells contain little or no chlorophyll. Traversing this tissue are numerous bundles of slender spiral vessels. The inner epidermis consists of flattened polygonal cells with oval stomata.†

The Microscope in the Diagnosis of Worms.—In the 'Birmingham Medical Review,' the first number of which has just appeared, there is an account of the use of the microscope for the above purpose by M. Bouchut, of Paris. M. Bouchut admits that we owe it originally to M. Davaine. The following brief account is taken from the 'Bulletin de Therapeutique':—"I have prepared for the microscope a small portion of excrement, in order that you may see how the diagnosis may be made, and that you may observe the form of the eggs of the lumbricus. (?) In the preparation you can see the eggs, oval in shape, with scolloped borders like a raspberry, and containing an inner sphere, in which are included some molecular granulations. The presence of these eggs in the fæces gives certainty to the diagnosis of the existence of worms in the intestines, and removes all hesitation about the employment of vermifuges."

The Microscopy of Tuberculosis.—Dr. C. F. Rodenstein has a long paper on this subject in the 'New York Medical Journal' for Decem-

* 'Dodonæa,' Part ii., p. 18.

† See also 'Transactions of Linnean Society,' vol. xxvii.

ber. Among his observations the following seems of interest. He says that in making experiments with blood corpuscles he has lately noticed that if a drop of blood, freshly drawn, be placed in an alkaline solution of carmine, the red corpuscles lose their power of forming rouleaux, and the white corpuscles absorb the carmine, seek each other, congregate in little masses, and seem to become agglutinated to each other. In a drop of blood prepared for microscopic inspection, by careful focussing there can be seen the whole field covered by fine little rings, which seem to form a delicate network, looking somewhat like the cornea of a fly seen with a low power; this is nothing but the red corpuscles of the blood which touch each other by their edges. Scattered over this delicate, pale network, there can be seen, here and there, little, bright red, cellular masses; these are the white corpuscles of the blood tinged with carmine. In specimens of pathological urine he has also seen sometimes, under the microscope, that pus-corpuscles have a tendency to approach each other, and to form adherent masses. Perhaps the formation of miliary tubercles takes place in a similar manner. The amœboid cells, when leaving the circulation, may bring with them a tendency to form into granulations, or the chemical or physical condition of the surrounding tissues may determine them to assume the form of minute nodules. We find sometimes, with these semi-transparent bodies, others rather of a fibrinous structure; they may represent a subsequent stage in the development of tubercularization. The lymphoid cells may have changed into fibrinous tissue by progressive metamorphosis. Virchow thinks that the fibrinous tissue represents the first stage of tubercular growth, which has not yet unfolded itself into the full-blown cellular tubercle. Langhaus, on the contrary, looks upon this formation as the full development of the cellular tubercle, and describes it as consisting of three zones, formed by the transformation of round cells into fibrillæ. Whether such bodies can be distinguished from minute fibromas, or whether the one or the other be the earlier stage of a continuous development, the tendency of all tubercular deposits to speedy decay has been universally recognized. Virchow says: "This structure, which in its development is most nearly related to pus, inasmuch as it has the smallest nuclei, and relatively the smallest cells, is distinguished from other more highly-organized forms of cancer, cancroid, and sarcoma, by the circumstance that these contain large voluminous corpuscles, with highly-developed nuclei and nucleoli. Tubercles, on the contrary, are always a pitiful production, and from the very outset perishable." And in this respect, also, tubercle betrays its origin and nature. The paper is of considerable length, and sums up the researches that have been made already on the subject.

The Young of Chironectes pictus.—A letter from Professor Agassiz is published in 'Silliman's American Journal of Science and Art' for February (1872), which is of great interest, as it tells us a good deal about the young of this fish. The letter is to Professor Pierce, and after some preliminary details of the nature of the nest, &c., which was found in the Gulf Stream, it says that a more careful examination very soon

revealed the fact that the elastic threads which held the Gulf-weed together were beaded at intervals, sometimes two or three beads being close together, or a bunch of them hanging from the same cluster of threads, or they were, more rarely, scattered at a greater distance one from the other. Nowhere was there much regularity observable in the distribution of the beads, and they were found scattered throughout the whole ball of sea-weeds pretty uniformly. The beads themselves were about the size of an ordinary pin's head. We had, no doubt, a nest before us, of the most curious kind; full of eggs, too; the eggs scattered throughout the mass of the nest and not placed together in a cavity of the whole structure. What animal could have built this singular nest was the next question. It did not take much time to ascertain the class of the animal kingdom to which it belongs. A common pocket lens at once revealed two large eyes upon the side of the head, and a tail bent over the back of the body, as the embryo uniformly appears in ordinary fishes shortly before the period of hatching. The many empty egg-cases observed in the nest gave promise of an early opportunity of seeing some embryos freeing themselves from their envelope. Meanwhile a number of these eggs with live embryos were cut out of the nest and placed in separate glass jars to multiply the chances of preserving them, while the nest, as a whole, was secured in alcohol, as a memorial of our unexpected discovery. "The next day I found two embryos in one of my glass jars; they occasionally moved in jerks, and then rested for a long while motionless upon the bottom of the jar. On the third day I had over a dozen of these young fishes in my rack, the oldest of which began to be more active, and promised to afford further opportunities for study. . . . . But what kind of fish was this? About the time of hatching, the fins of this class of animals differ too much from those of the adult, and the general form exhibits too few peculiarities to afford any clue to this problem. I could suppose only that it would probably prove to be one of the pelagic species of the Atlantic, and of these the most common are Exocœtus, Naucrates, Scopelus, Chironectes, Syngnathus, Monacanthus, Tetraodon, and Diodon. Was there a way to come nearer to a correct solution of my doubts? As I had in former years made a somewhat extensive study of the pigment cells of the skin, in a variety of young fishes, I now resorted to this method to identify my embryos. Happily we had on board several pelagic fishes alive, which could afford means of comparison, but unfortunately the steamer was shaking too much and rolling too heavily for microscopic observation of even moderately high powers. Nothing, however, should be left untried, and the very first comparison I made secured the desired result. The pigment cells of a young *Chironectes pictus* proved identical with those of our little embryos."

The Anatomy of the Pennatulians.—This continues to be admirably given in Professor Kölliker's fine treatise, which is being published at Frankfort. The work, of course, is not yet completed.

A Rare Fungus has been found and presented to the Manchester Philosophical Society (December) by the Rev. J. E. Vize, M.A., of

Forden, near Welshpool. The present was a slide of *Xenodochus carbonarius*, Schl., and Mr. Vize reported that this rare fungus occurs near Welshpool, in a railway cutting, with a south-westerly aspect, well sheltered by a hill and a wood. The first appearance on the leaves of *Sanguisorba officinalis*, L., was noticed in the middle of May, when the Lecythea-form was in perfection, but the stems and other portions of the Burnet were greatly distorted by it. A month afterwards the magnificent vermilion-coloured spores were well sprinkled over the leaves, the form of which was unaltered. In the middle of July the intensely black brand spores made their appearance, many of which had twenty or more articulations, and were plentifully scattered over the leaves in tufts. Mr. Vize stated that he had not watched the transition state from the Uredo to brand-spores, but he hoped to do so if opportunity offered.

Action of Quinine on the White Corpuscles of the Blood.—This subject, in which Binz and Stricker held somewhat different views, has recently, says the 'Lancet,' been taken up by Herr Kerner, who contributes a paper on this subject to the last part of Pflüger's 'Archiv,' being incited by the observations of Mosler on the cure of certain cases of leucæmia by the administration of quinine. Kerner remarks that it is quite possible to obtain a neutral salt of quinine, and in his experiments he used the chloride and the carbonate. He drew small quantities of blood from cats and dogs, and applied a one-tenth solution of this salt in proportion to the blood of 1 part to 4000, upon a microscopic stage maintained at blood heat. The result was striking. The white corpuscles became round and darkly granular, and the movements were very speedily completely arrested. It of course became interesting to compare these effects with those produced by other neutral salts, and in pursuing this investigation to some extent he found that salicin, caffein, atropine, and arseniate of potash were all either wholly indifferent or possessed only the slightest influence. Quinine therefore exerts a remarkable action on the white corpuscles of the blood, independent of its antiseptic properties.

The importance of Angular Aperture being stated in objectives, has been pointed out of late by several of our correspondents; but the remarks of one who signed himself "B" have been taken up by the 'American Naturalist' (January, 1872). This journal says that the estimation of any angular aperture, so well expressed by "B," is perfectly familiar and undisputed among experienced microscopists, although its exact bearings are not always easily apprehended by beginners; and that microscopists need occasional caution in regard to it may be inferred from the case in point, where an accomplished writer stated an extraordinary performance of a lens without mentioning the range of its apertures or the aperture at which he worked it. The peculiar and entirely independent qualities of lenses of low and of high angles are everywhere understood alike; but the extent to which success has been attained in this country in the construction of high angles cannot be appreciated abroad when "B," evidently well informed on other points, would not be surprised to hear that a one-

fifth of excessive resolving power had an angular aperture of 150° or 160°. Anyone in this country would be "surprised" to hear that its highest angle was less than 170°. The 'Naturalist' thinks that makers should always engrave the angular aperture upon the mounting and on the boxes of their objectives. The neatness and sufficiency of this plan, however, is marred in the case of many modern objectives whose screw-collar adjustment gives a wide range of powers and angles. Exactly at what point of adjustment the measurements should be made in these cases is one of the most difficult points to be settled in endeavouring to obtain a uniform nomenclature in regard to the works of different makers. At least for the present, until some standard degree of adjustment can be agreed upon, both the highest and lowest figures should be given where the range is considerable.

Lymph Follicles in Vagina.—The 'British Medical Journal' says that Lowenstein* shows that the mucous membrane of the vagina is not destitute of lymph follicles, as is generally asserted in anatomical text-books.

British Ostracoda.—We wish to direct attention to the fact that Mr. G. S. Brady has had issued, by the Linnean Society, his splendid 'Monograph of the Recent British Ostracoda.' It is a work which takes its rank with the fine volume of Baird, and the more thorough memoirs of Lilljeborg, Fischer, Zenker, Claus, Sars, and others. The classification is that proposed by G. O. Sars, son of the distinguished Norwegian zoologist, Prof. Michael Sars, in his 'Oversigt af Norges marine Ostracoder,' published in 1865. The Ostracoda are represented by the little two-shelled water-fleas, about half a line or less in length, which swim over the bottom or creep over submerged plants. As remarked by the author, "the geographical and bathymetrical distribution of the Ostracoda is a matter of the greatest interest as illustrating the probable condition under which the various fossiliferous strata have been deposited."

The Blind Fishes of the Mammoth Caves of America.—One of the best papers we have seen for a very long time in the 'American Naturalist' is that on the above subject, by Mr. F. W. Putnam, in the January number. It is too long for an abstract here, but we specially commend it to our readers' notice.

Stauropteris Oldhamia.—At a meeting of the Philosophical Society of Manchester (January 9th), Mr. E. W. Binney, F.R.S., exhibited some specimens of a fossil plant resembling the *Psaronius Zeidleri* found in the Upper Foot Coal Seam, near Oldham. This species has been described by Corda, in his 'Beitrage Zur Flora Der Vorvelt,' and figured in plate xi., but has not hitherto, he believed, been met with in the British coal-fields. The Oldham specimen appeared to him to be a petiole, of about one-eighth of an inch in diameter, and is of a nearly circular form in its transverse section, two-thirds of it consisting of a zone of strong parenchymatous tissue and an internal axis of vascular tissue arranged in four radiating arms of an irregular oval

* 'Centralblatt,' 35, Sept. 2nd, 1871.

form, resembling a St. Peter's cross. As he could not connect the specimen with a stem of *Psaronius*, he proposed to call it *Stauropteris Oldhamia*. In the above-named coal, as well as that of the Lower Brooksbottom Seam, there is a great variety of beautiful petioles which have not yet been described. Some of them evidently belong to the genus *Zygopteris*, and may probably be discovered in connection with their stems, but most of them have been found detached, and sometimes mistaken for the rootlets of *Stigmaria*. From some specimens in his cabinet he is led to believe that Cotta's *Medullosa elegans* is merely the rachis of a fern or a plant allied to one.

NOTES AND MEMORANDA.

Death of Mr. Patterson, F.R.S.—We regret to announce the death of Robert Patterson, F.R.S., which took place on the 14th of February, at Belfast. He was well known as a distinguished zoologist and as the author of Patterson's 'Zoology for Schools.' He did much service in national education. His microscopic work was not of equal importance with his general zoological researches.

The 'Amateur Microscopist' is the title of an American work just published. It is by Professor John Brocklesby, M.A., and is illustrated by 247 figures on wood and stone. We have not yet seen it, so of course we retain our opinion of it.

A Text-book of Pathological Histology.—This valuable book, which is by Professor Edmund Rindfleish, of Bonn, has been translated into English from the second edition by Dr. W. C. Kloman and Dr. F. T. Miles, both American physicians.

Mr. Darwin in the Academy of Sciences.—We are glad to learn from the 'British Medical Journal' that Mr. Darwin has been placed first on the list for the forthcoming election of a Corresponding Member in Zoology of the Academy of Sciences of Paris, and will therefore no doubt receive the honour. His supporters were MM. Milne-Edwards, Quatrefages, and Lacaze-Duthiers.

Excursion of the 'Hasler.'—We learn from an American contemporary that this vessel sailed on its voyage of discovery on December the 4th, and will touch first at St. Thomas Island. The steamer burns less than three tons of coal a day, and can thus run 8000 miles on 150 tons of coal, a remarkable saving of fuel. Professor Agassiz has taken out abundant stores for preserving specimens, and deep-sea nets and hooks specially adapted for catching fish at great depths. This journal (the 'American Naturalist') also publishes a letter to Professor Pierce, the Superintendent of the Coast Survey, in regard to the aims of the dredging party.

A Paper on the Œsophagus of the Hornbill (*Toccus melanoleucus*) has been presented to the Zoological Society at one of its January meetings, by Professor Gulliver, F.R.S. It was an appendix to a former paper by him on the taxonomic character of the muscular sheath of the œsophagus of the Sauropsida, read at a previous meeting of the Society.

A Substance like Ambergris, but which was clearly not that substance, was exhibited at a late meeting of the Lyceum of Natural History of New York. Professor A. M. Edwards, however, said that this substance had been presented several years since at a meeting of the Lyceum by a gentleman of the name of Southworth, who owned a large tract of land near Bahia, in Brazil, where it occurred in large quantities. At that time it had been referred to him for examination and report. He had determined that it was deposited in now extinct lakes beneath whose beds were bitumen springs, the lighter oils from which substance had infiltrated into and impregnated the mud. Sometimes the roots of plants, the remains of leaves, and even wood were found imbedded in it, but no diatoms or other microscopic organisms, by means of which the character of the water beneath which it had been deposited could be determined. The owner proposed, and in fact had to some extent used it for the production of gas for illuminating purposes, as the town of Bahia had been in this way lighted. If it had not been so light and bulky, it had been proposed to ship it abroad for distillation.

Prizes for Amateur Microscopists.—We believe the prizes mentioned beneath are now open to Fellows of this Society. They are as follows:—(1) The Countess of Ducie's Prizes for the best lists of the ponds and other aquatic resorts for collecting purposes, within 20 miles of Charing Cross. 1st Prize, Three guineas. 2nd Prize, Two guineas. Rules:—1. The exact locality of the pond must be given, in order that it may be identified, and the name of the railway station nearest to it.—2. Each competitor to send in his lists sealed in a cover bearing a motto, and accompanied by an envelope sealed, in which is enclosed the real name, address, and occupation of the competitor. These are to be delivered not later than March 31, 1873, addressed—Secretary, Natural History Prizes, 100, Fleet Street, E.C.

(2) The Countess of Ducie's Prize for the best list of the ponds and other aquatic resorts for collecting purposes, within 20 miles of Charing Cross, with a list of the microscopical animals and plants found in them during each month of the year, commencing March 1, 1872. Five guineas. Rules:—1. The exact locality of the pond must be given, and the name of the nearest railway station.—2. The date of the visit must be specified.—3. When any rare or supposed new objects are found, specimens should be immediately forwarded to Walter W. Reeves, Esq., Royal Microscopical Society, King's College, Strand (Somerset House), for examination.—4. Each competitor to send in his lists and other information sealed in a cover bearing a motto, and accompanied by an envelope sealed, in which is enclosed the real name, address, and occupation of the competitor. These are

to be delivered not later than March 31, 1873, addressed—Secretary, Natural History Prizes, 100, Fleet Street, E.C.

N.B.—Importance will be attached by the Adjudicators to any *Notes and Records of Pond Life* made during each month of the year with reference to the *Local Distribution, Development,* or *Hybernation of the Species.*

The information might be sent in monthly, if preferred: such information would be considered confidential. At the close of the year the series of monthly reports thus sent in would be treated as the competition for the prize.

Adjudicators—Henry J. Slack, Esq., F.G.S., Secretary of the Royal Microscopical Society, author of 'Marvels of Pond Life,' &c., &c.; Walter W. Reeves, Esq., F.R.M.S., Assistant-Secretary to the Royal Microscopical Society.

CORRESPONDENCE.

Peculiarities of Resolution.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, Mass., U.S.A., *Dec.* 29, 1871.

Sir,—I notice in your Journal for December that Mr. Wenham admits that in the illustration I have given in your number for November, that the rays are transmitted through the balsam-mounted object "in all directions." Mr. Wenham doubts the possibility of producing a set of combinations *above* the spherule to transmit a pencil of even 90°. Of course the accomplishment of this last singularly-supposed impossibility settles the question against his claim.

Well, allow me to say I have had constructed a low-power *dry* objective of clear focal distance of about $\frac{1}{10}$-in., and of 100° angular aperture at the point for *uncovered* objects,—open point, in other words. Before this objective I have placed "a little spherule" composed of two hemispherical lenses, and a thin film of fluent blood between. Easily and quickly done, no "cleverness." When the blood is between in a fluent state, the angular aperture measures the *same* as the dry objective alone. When the blood has dried, the angle contracts to 82° (+). Now it is evident enough that the liquid blood between the plane surfaces of the hemispheres opens the angle of transmission through the spherule to about 120°, all rays beyond being "totally" reflected. When the blood has dried, the ordinary case of total reflexion at *interior* plane surfaces obtains, and the pencil is cut down to 82° (+), *i. e.* incidence of 41° (+), at the *plane surface* of the lower hemisphere of "the little spherule."

Between Mr. Wenham and myself, of course this concludes the case. But for the benefit of students fresh to practical optics, I

propose (probably in your March number, if you will publish the proposed article) to set forth the applicability of the illustration to all immersion objectives. My proposition is that the balsam-mounted object can, in the case of the immersion objective, be viewed with the advantage of an angular pencil actually transmitted through the object of approximately 170°, more or less. But it should be *particularly noted* that this is not avowed for *water* as the medium *instead* of *air* above the slide-cover, but instead of air a medium approaching closely the refractive power of glass. Between immersion and dry objectives this is the remarkable difference, with the *latter* only 82° (+) can be obtained. This Mr. Wenham long since showed to be the case, and I would humbly remark that I was aware of this and of Dr. Robinson's demonstration before in " simple honesty " giving my first illustration in your Journal for July last, and to which Mr. Wenham so stoutly took exception.

Respectfully yours,

Robert B. Tolles.

P.S.—Allow me to state as a fact of interest to microscopists that the resolution of *Amphipleura pellucida is not* (or any longer) a difficult achievement. With a true $\frac{1}{10}$-in. of only 90°, *even less than* 75°, I have repeatedly and plainly lined the valve, *using sunlight and blue cell*, the specimen of *A. pellucida* being one kindly supplied to me as a proper test specimen by Dr. J. J. Woodward, and received by him from London. This performance was not dependent on any special merit in the objective—any good immersion $\frac{1}{10}$th would do the same thing. To accomplish this I used a narrow angled 1 in., 10° swinging under the stage of the microscope as a *condenser*. This 1-in. was placed at the incidence (obliquity to the axis of Mic.) indicated above, *viz.* 45° *or less*. The focus for parallel (sun's) rays a *little* outside of the object.

R. B. T.

On Positive and Negative Aberration.
By Dr. Royston-Pigott.

To the Editor of the 'Monthly Microscopical Journal.'

Mr. Editor, 2, Lansdowne Crescent, *Feb.* 7, 1872.

Sir, — As my paper printed in the Transactions of the Royal Society has been honoured by criticisms by two of your contributors, professing to point out errors of a mathematical nature, undetected by the accomplished mathematicians who scrutinized it, you will, I trust, permit me to observe that in a standard optical work the following passages occur, in which some of our Fellows may perhaps be interested :—

" We suppose in these pages light to proceed from *right* to *left*, and *positive* lines consequently to be measured from *left* to *right*.*

In the case of a converging lens Dr. Parkinson shows, as also

* Dr. Parkinson's, F.R.S., 'Optics,' page 13, 2nd Edition.

does Coddington (both Senior Wranglers), the aberration of a convergent lens is *negative*, the expression for which commences with the negative sign (−). Thus: "The aberration of the ray of S = $v' - v$ (page 119)

$$= -\frac{\mu - 1}{\mu^2}\left\{\left(\frac{1}{r} - \frac{1}{u}\right)^2\left(\frac{1}{r} - \frac{\mu+1}{u}\right) - \left(\frac{1}{s} - \frac{1}{v}\right)^2\left(\frac{1}{s} - \frac{\mu+1}{v}\right)\frac{v^2\,y^2}{2}.\text{"}\right.$$

The distasteful sentence is thus given in the Transactions of the Royal Society, in a note, vol. ii., 1870:—

"It is convenient to define the aberration to be positive or negative, or the lens to be *over-* or *under-* corrected, by the simple fact that a convex lens causes the excentrical rays to cross the axis at a point nearer the centre of the lens than the centrical rays; in which case, and in all analogous cases, it may be said that the lens is under-corrected and afflicted with a negative aberration."

The language of mathematicians unfortunately somewhat differs on this point from the rule of thumb employed in the workshop. In mathematical language, if a line is measured to the right from a fixed point be called *positive*, that measured to the left is *negative*. Accordingly as the marginal rays of a convex lens fall nearer to it than the central rays, the aberration is called *negative*. Any ordinary convexo-plane, plano-convex or double convex, is said in mathematical language to leave a negative aberration, and this lens is under-corrected and *vice versâ*.

Now, Sir, as it is due to myself to give this explanation, after the manner in which this statement has been treated, I may be allowed to draw your attention to the construction of the *positive* and *negative eye-pieces*, and especially to the principles upon which these names have been bestowed by mathematicians upon these combinations.

I am, Sir, your obedient servant,

G. W. Royston-Pigott.

Dr. Pigott and Sir David Brewster.

To the Editor of the 'Monthly Microscopical Journal.'

February 12, 1872.

For the credit of the Microscopical Journal, and in justice to the scientific character of the late Sir David Brewster, Dr. Royston-Pigott, upon having the matter pointed out, may probably offer some explanation as to certain statements he is reported to have made in the current number of your Journal, at page 87.

Dr. Pigott, referring to photography, states that "Sir David Brewster has laid down the principle that you cannot get a true picture if you use a very large object-glass upon a small object, on account of certain distortions, which, according to Sir David's principle, are caused by the mixing up together of all the images caused by *different areas* of the lenses, the result being a compound image but not a likeness:" he adds that, "to show the portrait cor-

L 2

rectly you must diminish the aperture of the glasses to that of the eye."

As Dr. Pigott has quoted Sir David Brewster, it is to be presumed he must be able to refer to some authority for his quotation, as it is hard to believe that Sir David Brewster would maintain that different areas of the surface of a lens corrected for spherical aberration would give separate or non-concurring images, the essence of a corrected lens or combination of lenses being that the whole area of its surface *concurs* in forming every part of the image at the focus. The fact just stated being demonstrable and well known, I presume it will be accepted.

But Dr. Pigott extends his doctrine to the improvement of the microscope, and if his fundamental principle were sound it would be equally applicable in both cases; but the fallacy of separate areas giving separate and *different* images of the same thing being demonstrable, it is unnecessary to pursue that matter farther. Dr. Pigott, however, in support of his argument, gives a microscopical experiment, which is not without interest. If a minute object be viewed under a glass of large aperture, according to Dr. Pigott "a great many views of the object will be produced, all different; yet all these views are mixed up into one compound image: the eye receives rays from every side of a minute object at once: the observer looks at it from innumerable points of view at one and the same instant:" and under these circumstances it is not to be doubted that very little could be seen. But, says Dr. Pigott, reduce the aperture of the glass, and the object will be seen in the greatest perfection; and here, I am happy to say, I am able so far to agree with him.

The truth is, comparatively few objects are seen to advantage under a large aperture; in most cases one half of the pencils of light cannot be brought into focus along with the other half, and the result is frequently "fog" and indistinctness.

With the large aperture the pencils have points of this shape ——| F, and only such parts of the image will be seen as may be *critically* in focus, as at F; while the pencils notin focus cause indistinctness. With the contracted aperture the points of the pencils are changed into this shape ——|+| (D E F), which gives a greatly extended range, or depth of focus, almost equally good at D, E, or F; and features are thus distinctly seen at one view which, with the large aperture, would be entirely out of focus, and invisible.

But this has nothing to do with the "separate images, all different," said to be formed by different areas of the microscopic object-glass.

G. S. Cundell, F.M.S.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society's Annual Meeting.

King's College, *February* 7, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, among which, the Secretary stated, were 107 slides of Australian zoophytes, presented to the Society by Mr. Maplestone, of Victoria. These slides had been all mounted by Mr. McIntire, who had expended a large amount of time and trouble in the work. Mr. McIntire requested that the specimens be submitted to some competent authority for examination and determination of species.

The Secretary then proposed a vote of thanks to the donor of the gifts referred to, and moved that a special vote of thanks be passed to Mr. McIntire for the time and trouble and care he had taken in dealing with the slides just mentioned. A vote of thanks was unanimously accorded to Mr. McIntire.

The Secretary announced that Dr. Wallich would be proposed for election as an Honorary Fellow of the Society, in consideration of services rendered to microscopical science, and of the handsome donations of objects which he had made to the Society's cabinet.

Messrs. Gay and Groves having been appointed scrutineers, a ballot was taken for the election of officers for the ensuing year; the scrutineers reported that the whole of the gentlemen proposed on the list had been elected.

The Reports of the Library and Cabinet Committee were read, and also the Treasurer's cash statement.

The President then delivered the Anniversary Address, which will be found in full at page 89.

Dr. Lawson moved, "That the thanks of the meeting be presented to the President, and that he be requested to print his valuable Address." Dr. Lawson said it was utterly impossible for any Fellow present to overrate the importance of the subject; its vastness excited their wonder and their admiration of the patience and skill displayed in its investigation.

Dr. Braithwaite seconded the motion with great pleasure. Notwithstanding the fact that few present could appreciate the real value of the President's Address, it was very evident that he had brought forward the essence of a very great amount of work in a small compass, of which he hoped to hear more in detail at some future occasion.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

The vote of thanks to the President, and the motion that he be requested to print his Address, were unanimously carried.

The President said he was very much obliged for the patience which the Fellows had exhibited during the reading of his Address; he was so unused to speaking or reading in public that he believed the very reading of his paper had weakened the effect of it. He hoped it would serve as the means of arriving at what is doing in reference to the subject. He was sure that if the young men would take up the study they would find it a most glorious field of work. The great thing to be done was to find out the facts of nature, and then everything would explain itself.

Mr. Hogg showed a specimen of "*Mycetoma*, or Fungus-foot of India," his report on which was taken as read.

The Secretary announced that at the next meeting a paper by Dr. Klein would be read "On the Earliest Stages of the Development of the Trout," and also a paper by Dr. Murie "On the Classification and Arrangement of Microscopic Objects."

Annual Report of the Society.

The Secretaries report that during the past year the Society has been well supplied with papers, extending over a great variety of subjects, and in many cases bringing forward new discoveries of interest and importance. It is found that scientific investigators appreciate the prompt publication of their researches, which the Society offers through the Monthly Journal.

The Society's collections of books, instruments, and objects may be generally described as in good condition; and, while scarcely any purchases have been made, valuable additions have been received in the donations specified in the subjoined list.

On the 10th of May and on the 24th of January two scientific evenings were held, and devoted to the exhibition of objects and apparatus of remarkable interest, and friendly conversation thereupon. On both occasions a considerable number of Fellows responded to the invitations of the Council and ensured success, both as regards the number, and character of the articles, preparations, &c., displayed, and many objects of interest were kindly lent by non-Fellows. It is intended to continue evenings of this description, of which due notice will be given; and it is hoped that each time an increasing number of Fellows will co-operate by bringing their microscopes, with objects of novelty or remarkable merit.

Books Purchased during the past Year.

Annals of Natural History. 2 vols.
Quarterly Journal of Microscopical Science. Vol. XIX.
The Handbook of British Fungi. 2 Vols.

Presented.	From
Traité du Microscope. Par Chas. Robin	*Author.*
Royal Society's Catalogue of Scientific Papers. Vol. V. ..	*Royal Society.*
Transactions of the Linnean Society. 3 Parts	*Society.*

Several Pamphlets and Papers, as well as the Journals of other Societies, in exchange for our own, have been periodically announced in the 'Monthly Microscopical Journal.'

APPARATUS AND SLIDES.

Microscope and quantity of Apparatus. By M. Chevalier, of Paris .. .. *Sir J. Sebright.*
Logan's Simple Microscope, with three powers .. .. .. *J. N. Logan, Esq., U.S.A.*
Spot Lens for Low Powers .. .. *Chas. Baker, Esq.*
229 Slides have been presented during the year, including a Möller's Diatomaceen-probe-platte. Given by the Rev. R. H. Nisbett Browne.
100 Vegetable Fibres, mounted and presented by W. T. Suffolk, Esq.
36 Palates of Victorian Mollusca and 107 Australian Zoophytes. Presented by C. Maplestone, Esq., of Williamstown, Australia, and kindly mounted for the Society by S. J. McIntire, Esq.
24 Slides of Intestinal Worms, sent over from the Agricultural Society of New South Wales.
12 Slides of Lepidopterous Scales, selected by the Chevalier de Cerbecq.

The financial state of the Society is shown in the Treasurer's account, appended hereto. It does not present any features calling for special remark.

RICHARD MESTAYER, TREASURER, IN ACCOUNT WITH THE ROYAL MICROSCOPICAL SOCIETY.

1871.	£.	s.	d.	1871.	£.	s.	d.
Balance from 1869	34	2	7	Paid Mr. Hardwicke for Journal	251	2	6
Received from Charter Fund	50	0	0	Rent, King's College, to Dec., 1870, and attendance ..	46	5	6
Dividend on 1059*l.* 6*s.* 2*d.* Consols to Jan. 5, 1871 ..	15	12	6	King's College, expenses of Soirée, 1870	34	10	5
Dividend on 1059*l.* 6*s.* 2*d.* Consols to July 5, 1871 ..	15	9	10	Books and binding, 1870–1	43	13	5
Composition	10	10	0	Stationery and printing ..	11	3	0
Subscriptions for 1869 ..	9	9	0	Reporter	9	9	0
" 1870 ..	49	7	0	Ray Society	1	1	0
" 1871 ..	357	0	0	Insurance to Christmas, 1872	2	8	0
" 1872 ..	5	5	0	Instruments	2	8	0
Screw tools sold	0	14	0	Petty cash	20	0	0
				Mr. Reeves' salary	63	0	0
				Collector	11	0	0
				Balance due from Treasurer	51	9	1
	£547	9	11		£547	9	11

Jan. 22, 1872. Examined and found correct,

JAMES HILTON, } *Auditors.*
HENRY PERIGAL, }

Sixteen gentlemen have been elected Fellows in the course of the year, and one Honorary Fellow.

During the past year the Society lost three Fellows by death,—John Harker, Edward Wilkinson, and Sir Roderick Murchison. John

Harker, Esq., F.R.M.S., was elected in 1857 and died November 16th, 1871. Of the death of Mr. Wilkinson the Secretaries failed to obtain any accurate record: he was elected in 1863. Sir Roderick Murchison, Bart., F.R.S., &c., became a Fellow of the Society in 1846, but it does not appear that his attention was at any time directed to microscopical research. He was, however, highly distinguished by his various contributions to geological science, and in particular by his great work, 'Siluria,' in which he treated of the whole of the deposits of the Silurian strata from his own close observation in all parts of Europe. This work, the last edition of which appeared in 1867, includes the materials of the Silurian system, with such additions and corrections as were needed to the above-named date, and is a monument of geological research. One of the points which has been settled from Sir Roderick's observations is the position of the rocks of the north-west coast of Scotland, which had been supposed to be old red sandstone, but were at length found to underlie rocks containing Silurian fossils, and therefore to be classed as Cambrian. These rocks were, in fact, found to be the oldest Cambrian, lying below the fossil-bearing Cambrians of the Welsh coast and of the east of Ireland, which were esteemed the lowest fossiliferous rocks until the discovery of organic remains in the Laurentian rocks of Canada, in the valley of the St. Lawrence.

Sir Roderick, having acted for five years as secretary of the Geological Society, became president of that body in 1831-2, and again in 1842-3. He was one of the founders and most active members of the British Association for the Advancement of Science; for several years he acted as general secretary, and presided over its meeting at Southampton in 1846. He has from year to year taken the leading part in the business of the Geographical Section, and has communicated many important papers. In 1844 he was elected president of the Royal Geographical Society, was re-elected in the following year, and again in 1852 and in 1856.

List of Officers and Council of the Royal Microscopical Society, Elected 7th February, 1872.*

President.—William Kitchen Parker, F.R.S.

Vice-Presidents.—*William Benjamin Carpenter, M.D., F.R.S., &c.; John Edward Gray, Ph.D., F.R.S., &c.; *Sir John Lubbock, Bart., M.P., F.R.S., &c.; John Millar, L.R.C.P. Ed., F.L.S.

Treasurer.—*John Ware Stephenson, F.R.A.S.

Secretaries.—Henry J. Slack, F.G.S., and Jabez Hogg.

Council.—Robert Braithwaite, M.D., F.L.S.; John Berney, Esq.; *Charles Brooke, M.A., F.R.S.; *Thomas William Burr, F.R.A.S.; William John Gray, M.D.; Henry Lawson, M.D.; Henry Lee, F.L.S., F.G.S.; *Samuel John McIntire, Esq.; *Henry Perigal, F.R.A.S.; G. W. Royston-Pigott, M.A., M.D., &c.; Charles Stewart, M.R.C.S., F.L.S.; Thomas Charters White, M.R.C.S.

* Those with the asterisk before their names were elected new members.

Scientific Evening.

On the 24th January the Royal Microscopical Society held a scientific evening for the exhibition of objects and apparatus of novelty or remarkable interest, and for friendly conversation thereupon. To avoid crowding no general invitations were issued, and the gathering, of about 200, was almost entirely composed of Fellows, with a few visitors of scientific eminence.

From the subjoined list of objects and articles exhibited it will be seen that the suggestions of the Council were effectually carried out by the zeal and kindness of Fellows and their friends, and it was observed with much satisfaction that many Fellows residing in various parts of the country came from long distances to be present on the occasion.

This opportunity for quiet observation and interchange of ideas upon important topics was thoroughly appreciated by those present, and it is the intention of the Council, as stated in the Annual Address, to hold similar evenings at convenient intervals, of which due notice will be given.

The Society was indebted to Mr. How and Messrs. Howe and Thornthwaite for the loan of excellent lamps.

The Society exhibited a specimen of Mr. Stanistreet's micro-ruling on glass, the lines $\frac{1}{1800}$th of an inch apart, and a beautiful specimen of cupric sulphate with colloid silica prepared by H. J. Slack, Esq.

Mr. C. D. Ahrens: Petal of geranium, shown under a $\frac{1}{4}$-inch objective, with a new kind of erecting binocular microscope.

Messrs. Beck: A unique specimen of cinnabar, with crystals of cinnabarite; and a Podura scale under their new $\frac{1}{10}$-inch objective.

Dr. L. S. Beale: Ultimate nerve networks and plexuses of pale compound fibres with their nuclei distributed to the unstriped muscular fibres of the frog's bladder, mounted March, 1863.

Dr. Bruce: White blood corpuscles, their passage through the capillaries and their amœboid state.

Mr. John Browning: *Acarus Crossii*, kindly lent by W. G. Lettsome, Esq. The spectrum of metallic indium with his Universal Automatic Spectroscope, and seeds of *Gerardia communis* with Mr. Stephenson's binocular microscope.

Dr. Carruthers: Traquairia, a newly-discovered fossil Radiolarian from the Coal-measures, supposed to be the first Radiolarian found below the Chalk.

Mr. Thomas Curteis: Parasite of elephant.

Dr. W. J. Gray: Section of finger with nail *in situ.*

Mr. Edward George: Fructification of Hepaticæ. The spores and elaters of *Scapania undulata*, &c.

Mr. F. W. Gay: The very pretty little Nudibranchiate mollusk from the Victoria Docks, *Embletonia Grayii.*

M. A. de Souza Guimaraens: Cysticercus from a rabbit.

Mr. James How: Two microscopes and his new lamp.

Dr. George Johnson: Hypertrophied muscular walls of arteries (chronic Bright's disease).

Mr. Henry Lee: Sponge spicules *in situ*, *Pheronema Grayii.*

Mr. W. T. Loy: Section of an entire blow-fly, *Musca vomitoria.*

Mr. S. J. McIntire: *Templetonia nitida* alive, the scale of ditto with Powell's $\frac{1}{8}$th and Wenham's truncated lens. The Test Podura mounted to show distinctive characters.

Mr. J. Needham: Human louse changing its skin, and a section of human lung injected.

Mr. L. Norman: A large frame of wood sections, and one of fossils from the Coal-measures.

Mr. Frederick Oxley: An erecting arrangement for dissecting, &c., under the binocular compound microscope, being a modification of the arrangement described by Mr. Ward, in No. 37 of the 'Monthly Microscopical Journal.'

Mr. W. K. Parker (the President): Some very beautiful slides of Foraminifera.

Mr. George Potter: *Alceonella stagnorum*, alive.

Messrs. Powell and Lealand: Podura scale, 3000 diameters, and *Pleurosigma angulatum*, 2000 diameters, under their new $\frac{1}{25}$th objective.

Mr. M. Pillischer: Some very fine injected preparations.

Dr. Royston-Pigott: A kratometer for taking micrometric measurements very readily and ascertaining the power of objectives.

Mr. Walter W. Reeves: Diatoms *in situ*, *Podosira Montagnei.*

Mr. Edward Richards: Two new portable lamps.

Messrs. Ross: Beaded structure of *Pleurosigma angulatum*, shown with Wenham's truncated lens and parabola under their $\frac{1}{10}$th object-glass.

Mr. W. T. Suffolk: Stellate hairs of *Pomidaris apetela*, N. O. Rhamnaceæ.

Mr. Charles Stewart: One of the Entomostraca, *Nebalia bipes*, polarized. Zœa stage of one of the crabs; the heart may be seen in its pericardium immediately beneath the dorsal spine. Globular dentine from the tooth of walrus, and two stages of the development of the hair and hair-follicle, *viz.* first, as an ingrowth of the deep layer of the epidermis; second, the cells in the middle of the ingrowth become modified to form the hair, the outer ones remaining to constitute the hair-follicle.

Mr. John W. Stephenson exhibited his new erecting binocular microscope, and the following beautiful specimens prepared by Dr. Klein:—Nerves and connective-tissue corpuscles of the cornea; nerves of the conjunctiva of rabbit; and a vertical section of skin of negro's head, with the hair-follicles greatly curved.

Mr. Charles Tyler: A series of sponge sections and sponge spicules from Barbadoes, Japan, Jamaica, Australia, India, North America, and the British Isles.

Mr. Thomas C. White: Preparations of the dental pulp and a curious form of hippuric acid polarized.

Donations to the Library and Cabinet from January 3rd to February 7th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Popular Science Review. No. 42	*Ditto.*
Journal of the London Institution, Nos. 10 and 11	*Institution.*
Transactions of the Linnean Society, Vol. XXVII. Part 4, and Vol. XXVIII. Part 1	*Society.*
Report of Surgical Cases in the U. S. Army. Circular No. 3	*Surgeon-General U.S.*
Journal of the Quekett Club	*Club.*
Two Slides of Crystals with Colloid Silica	*H. J. Slack, Esq.*
107 Slides of Australian Zoophytes sent over by C. Maplestone, Esq, and mounted for the Society by S. J. McIntire, Esq.	
Six Injected Preparations	*Moritz Pillischer, Esq.*
Nine Slides of Insect Scales	*T. W. Wonfor, Esq.*
A Polarizing Medium	*Thos. Armstrong, Esq.*

The following gentlemen were elected Fellows of the Society:—

Charles Gibson, Esq.
David Johnson, Esq.
William Boyd Moss, M.D.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

January 11th, 1872.—This Society, which has been in existence for eighteen years, and during that time done good service in stimulating research and in promoting scientific discovery, gave its first invitation soirée to the members and their friends, some 200 of whom met in the Library and Board-room of the Dispensary.

The walls and tables of both rooms were covered with a large and interesting selection of specimens, drawings, and photographs, illustrative of each branch of Natural History, in the hanging and classification of which efficient service was rendered by Messrs. R. Glaisyer, Penley, G. Scott, Saunders, C. P. Smith, T. W. Wonfor, and Walsh.

The leading objects exhibited in the Library were some very beautiful cases of British birds, collected and mounted by Mr. H. H. J. Nicholls; a white cock pheasant and male and female little gull, in mature plumage, by Mr. Pratt; a fine collection of shells, by Mr. R. Glaisyer; a very curious series of Brighton beach pebbles, containing choanites, by Mr. Glaisyer; fine series of copper ores from the Burra-Burra Mine, by Mr. Wonfor; chalk and other fossils, by Messrs. Saunders and Dennant; specimens illustrating the Post-Pleiocene at Brighton, by Mr. J. Howell; vertebræ of Iguanodon, from the Isle of Wight, by Mr. J. Sewell; rubbings from Egyptian monuments, by Mr. Nourse; female white ant, crocodile, &c., by Mr. E. Moore; while the walls were covered with geological, ethnological, and other diagrams, lent by Mr. Wonfor.

In the Board-room a central table was occupied by microscopes contributed and presided over by Dr. Hallifax and Messrs. Glaisyer, Gwatkin, Marshall Hall, Turner, Sewell, C. P. Smith, Nash, and Wonfor.

The most striking objects exhibited in this room were a fine collection of British butterflies and moths, by Mr. W. Wonfor, jun.; Indian butterflies, moths and beetles, by Mr. Wills; a series of wasps and wasps' nests, by Dr. Ormerod; emerald in quartz rock, from South America, and emerald butterfly, said to be found only in the neighbourhood of the same mine, by Mr. Curtis; Japanese silkworms, moths, and cocoons, by Dr. Badcock; about 150 polished specimens of woods used in manufactures, by Mr. Saunders; series of British mosses and lichens, and enlarged drawings of fructification and dissections, by Mr. C. P. Smith; a very fine collection of ferns and lycopodiums, by Mr. Walsh; New Zealand ferns and sea-weeds, by Mr. Penley; double cocoa nut, by Mr. Hollis; petroleum, in all its forms, from shale and crude black oil up to white wax and manufactured articles, by Mr. Nash; case of saxicava and pholas, taken from burrows in hard limestone—the perforated rocks also shown — by Mr. E. Charlesworth; *Pheronema Grayii*, newly-discovered silicious sponge, dredged off the Portuguese coast, by Mr. Marshall Hall; *Euplectella speciosa*, another silicious sponge, by Mr. Sewell; flying fox, from Australia, by Dr. Badcock; large tusks of walrus and hippopotamus, by Mr. Parkinson; specimens of saw fish, cow fish, and parrot fish, by Mr. Sewell; a fine series of flint implements, collected recently by Dr. Stevens, Messrs. Wonfor, H. Saunders and Hilton, at St. Mary Bourne, Cissbury, Portslade, and the immediate neighbourhood of Brighton; polished stone weapons, found in England, Canada and the West Indies, bronze celts, axes, &c., chiefly found in Sussex, Early British, Saxon and Norman gold and silver coins, by Mr. G. Scott; bronze paalstabs, by Mr. R. Glaisyer; Roman vase, found in a garden on Round Hill, by Mr. Wonfor; series of photographs, illustrative of food products, eggs of parasites, diatoms, Nobert's lines, &c., by Mr. T. Curties (Holborn); very beautiful micro-photographs, by Drs. Hallifax and Maddox and Mr. Hennah; about 150 coloured plates from Hedwig's original drawings of mosses, fungi and lichens, with enlargements of parts and organs, by Mr. Wonfor; a very ingenious hygroscope, made from the awn of an erodium, which was delicately sensitive to variations of moisture in the atmosphere, by Dr. Hallifax; &c., &c.

Tea and coffee were provided during the evening, which was one of enjoyment to all present.

Previous to the soirée an ordinary meeting was held, when Messrs. Shireff, J. Wood, Infield and Howell were elected ordinary members, and Miss Hill a subscriber to the library.

Mr. Wonfor announced the receipt of 'The Climate of Uckfield,' by Mr. C. L. Prince, from the author; the 'Eighth Annual Report of the Belfast Naturalists' Field Club,' and 'Proceedings of the Eastbourne Natural History Society,' from the Secretaries. Votes of thanks were given to the donors.

Reading Microscopical Society.*

October 10th, 1871.—Captain Lang presided, and, after the usual business, read a short paper entitled "Another Hint on Selecting and Mounting Diatoms," in which he more particularly described a method, devised and adopted by Mr. Tatem, for obtaining separate diatoms from balsam-mounted slides. Specimens of diatoms taken from old slides and remounted were shown as illustrations of results.

He also exhibited a new form of wooden slide, with sunk cell, for opaque objects. This was obtained from Mr. Crouch, of London Wall.

Mr. Tatem exhibited arranged mounts of diatoms obtained from mixed and dirty balsam-mounted slides, and also specimens of *Difflugia spiralis.*

Mr. Austin exhibited as specimens of microscopic fungi, *Mucor ramosum*, *Phragmidium obtusum*, *Puccinia syndenesiarum*, &c.

Mr. G. Davies exhibited Möller's type slide.

November 7th, 1871.—Mr. Tatem contributed a short paper "On the Conjugation of *Amœba*," which appears in this Journal.

Captain Lang exhibited twelve slides of diatoms, each containing a separate species, shown in its two principal aspects of front and side views. The genera and species were *Biddulphia aurita*, *B. Tromeyi*, *B. pulchella*, *Gomphonema geminatum*, *Triceratium favus*, *Melosira arenaria*, *Navicula clepsydra*, *Pinnularia lata*, *P. alpina*, *Campylodiscus costatus*, *Surirella splendida.*

Mr. Tatem exhibited diatoms from Rio Janeiro and Puerta Segura.

December 5th, 1871.—Captain Lang exhibited, mounted side by side, on the same slide, the two valves of an entire frustule of *Surirella fastuosa*, picked from Singapore shell cleanings, and separated from each other under the microscope. He drew attention to the great dissimilarity of the two valves, the median space of the one being broadly lanceolate, whilst that of its fellow is linearly lanceolate; the costæ of one valve being, of course, proportionately shorter than those of the other, for he does not consider the irregular markings within the median space to be in either case the continued costæ, though Dr. Greville has described them as such. They appear to him entirely disconnected.

It has long been a well-known fact that the upper and lower valves of certain diatoms, as *Achnanthes* and *Cocconeis*, are not identical in structure, but he is not aware that this has been found to be the case in any of the very numerous species of *Surirella.*

Mr. Kitton, in his controversy with the Rev. E. O. Meara in the 'Quarterly Journal of Microscopical Science,' vol. viii., N.S., justly remarks upon the great difference in the size, outline, and striation of this particular species, but does not allude to the diversity in the valves of the *same* frustule.

Captain Lang also exhibited wings and elytra of rare butterflies and beetles from Brazil, sent to him by Captain Perry.

Mr. Tatem exhibited mounted diatoms, and Mr. Austin small fungi and mounted desmids.

* Report furnished by Mr. B. J. Austin.

South London Natural History Club.

The first annual soirée of the South London and Natural History Club was held on November 30th, at the "Horns" Assembly Rooms, Kennington Park, and proved a gratifying success.

Notwithstanding the adverse circumstance of inclement weather, nearly five hundred ladies and gentlemen were present during the evening, and appeared to appreciate the efforts to delight them, put forth by those who contributed towards the display. In addition to the members of the South London, representatives of the following associations exhibited:—The Quekett Club, the Croydon Club, the Sydenham and Forest Hill Club, together with numerous other friends and the under-mentioned opticians,—Messrs. R. and J. Beck, Bailey, Baker, Collins, Horne, Thornthwaite, How, Moginie, Richards, Stanley, Steward, and Swift.

Dr. J. D. Hooker, F.R.S., contributed four Californian photographs, and Mr. W. W. King displayed a large collection of photographs from the various plant-houses and gardens at Kew. Mr. J. D. Russell and Messrs. Dawes of Camberwell exhibited cases of preserved specimens; and a triangular aquarium, the invention of a member of the South London Club, attracted much attention. The personal welfare of the exhibitors was cared for by a specially-appointed committee, and at intervals musical performances were given by various professional and amateur friends.

The living objects shown were comparatively few in number; but *Conochilus*, *Volvox*, *Carchesium*, *Embletonia*, *Opalina* (a parasite from the frog), and various rotifers and water-fleas were to be seen.

The club may be congratulated on the unequivocal success of the first conversazione, nothing having transpired to mar the arrangements.*

An Ordinary Meeting of this Club was held at Glo'ster Hall, Glo'ster Place, Brixton Road, on Tuesday evening, January 16th, 1872. Mr. Deane, F.L.S., presided.

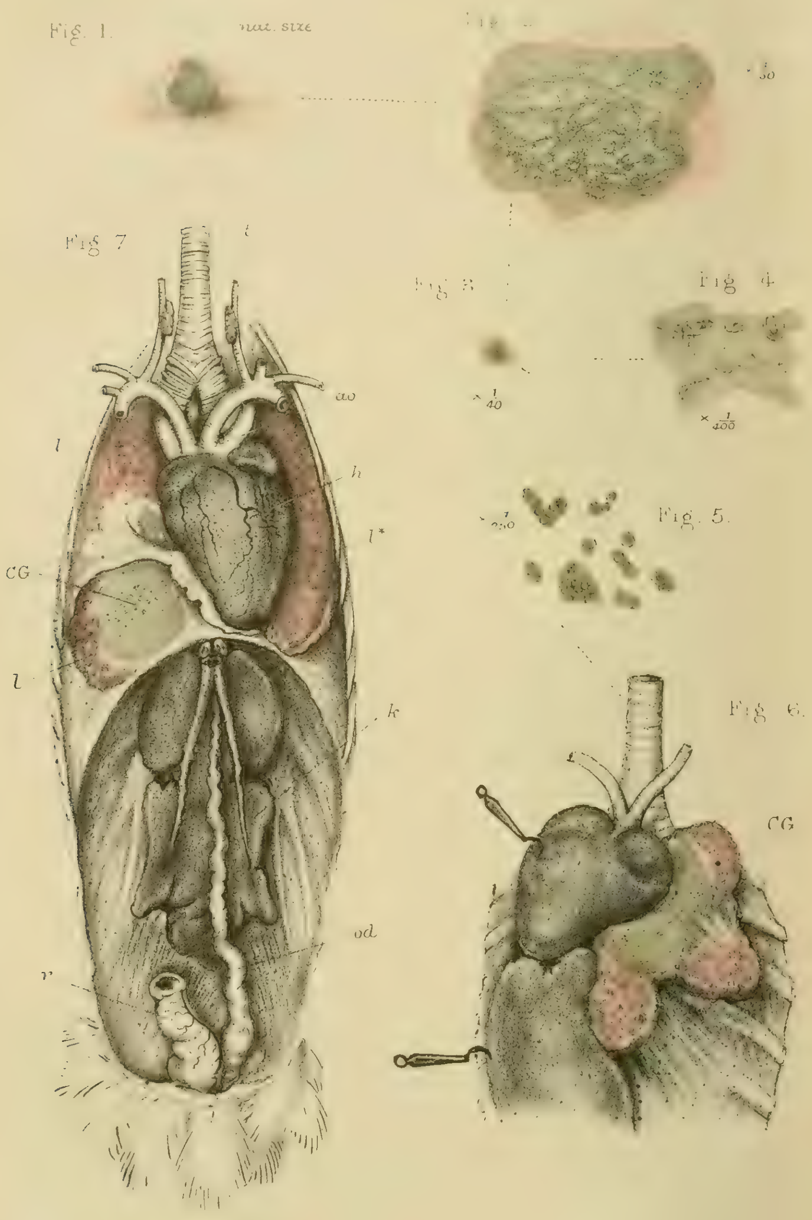
Fifteen gentlemen, proposed as members, were balloted for, and duly elected.

Mr. Bott, F.G.S., then read a paper "On the Chalk Formation," which, being in no way microscopic, we of course cannot publish in this Journal.

After it was read, a vote of thanks was unanimously accorded to Mr. Bott for his valuable paper.

The meeting then resolved itself into a conversazione; a paper having been announced for the next meeting, on Tuesday evening, February 20th, by Mr. Stewart, M.R.C.S., "On the Application of Polarized Light to Microscopical Subjects."

* See 'South London Press,' &c., Dec. 9, 1871.



C. Berjeau del. et lith. J. Murie. W. West & Co. imp.

Vegetable Organisms on Pleura of Living Birds.

THE

MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1872.

I. — *On the Development of Vegetable Organisms within the Thorax of Living Birds.** By Dr. James Murie, F.L.S., F.G.S., &c.; Lecturer on Comp. Anat. Middlesex Hosp.; formerly Pathol. Glasg. Roy. Infirmary; Assist. Conservator Roy. Coll. of Surg., Eng.; and late Prosector Zool. Soc., Lond.

(*Read before the* Royal Microscopical Society, *Jan.* 3, 1872.)

Plate XII.

The fact of lowly-organized vegetable structures finding a nidus and flourishing in the bodies of animals and man, is one well known to microscopists, physiologists, and pathologists. When these microscopical parasites, Algæ or Fungi, as the case may be, effect a lodgment on the exterior of the body, whether of the lower or higher animals, they not infrequently give rise to peculiar, tegumentary diseases. Such, for instance, are,—the forms of Favus

EXPLANATION OF PLATE XII.

Fig. 1.—Portion of the vegetable organism and its plastic membranous nidus from the pleura of the Great White-crested Cockatoo, *Cacatua cristata*, Linn. Sketched when fresh and of natural size.

„ 2.—Magnified view of part of same examined under a low power, and showing filamentous threads (mycelium) and echinulate spores.

„ 3.—Crucial arrangement of two filaments, at the junction of which a spore is attached.

„ 4.—Some of the cells and spores of the above highly magnified.

„ 5.—Sporules and basement membrane from cryptogamic growth in the Rough-legged Buzzard, *Archibuteo lagopus*, Gm.

„ 6.—Anatomical sketch of the thorax of the Rough-legged Buzzard, the heart being dragged to the right side to expose the cryptogamic vegetation on the surface of the lung.

„ 7.—The opened thorax and abdomen of the Kittiwake Gull, *Rissa tridactyla*, Linn. Designed to illustrate the natural position of the fungus patch towards the visceral organs; the gullet, stomach, liver, and intestines having been removed.

The lettering to this and preceding figure is as follows:—cg, cryptogamic growth. *l l* right, and *l** left, lung. *h*, heart. *ao*, aorta and other vessels. *t*, trachea. *k*, left kidney. *od*, oviduct. *r*, rectum.

* An abstract of this paper was laid before the Biological Section at the meeting of the British Association in Edinburgh, August, 1871.

of the dermatologists; the matted condition of the hair peculiar to the Polish people, hence termed Plica Polonica; the "fur"-like exudation from the bodies of flies;* the "mould" of gold-fish;† and many others more or less known, but too numerous to mention.

But while for the most part the vegetable microscopical parasites have a location on the surface of the body, it by no means follows that the internal organs are free from them. On the contrary, they abundantly flourish in the whole length of the alimentary canal.

Besides many well-known varieties developed in the mouth, gut, &c., one peculiar form from the stomach, *Sarcina ventriculi*,‡ has been described by the late Professor Goodsir § and others.

It may be remarked, that in the greater number of instances where minute vegetable organisms attach themselves to interior parts and organs, these have directly or indirectly communication with the atmosphere; and hence their production is accounted for by aerial conveyance of the spores.

A fresh interest accrues in those cases where epiphytes arise within closed cavities of the animal body, and as it were spring hap-hazard from dense living tissues. Does such phenomena bear upon those doctrines of "spontaneous generation" which have been so noisily bruited of late? At all events, here the mode of entrance of the germs, their subsequent development, botanical characters as elucidated by the microscope, relations in different animals, tissues attacked whether primarily diseased or caused by the parasitical implantation, are all questions of fundamental importance.

Perfectly aware of shortcomings and how many problems remain to be solved as respects the life history of these cryptogamic plants in deep-situate vital parts, I yet do not hesitate to place on record some limited observations *en passant*. This, partly to give a faithful anatomical sketch of the vegetation *in situ* (a desideratum in cases of kind), and partly to stimulate others to further researches in a field truly within the sphere of the members of this Society.

Three instances have come under my own immediate inspection, in which a vegetable-like mould has been developed within the thoracico-abdominal cavity of members of the feathered tribe; and the position and microscopical characters of which I herewith describe.

1. An adult male specimen of the Great White-crested Cockatoo, *Cacatua cristata*, Linn., was received in exchange by the Zoological Society, on the 9th May, 1868, and died on the 6th June following.

* See Dr. Ferdinand Cohn's admirable memoir, 'Empusa muscæ und die Krankheit der Stubenfliegen.'

† Dr. Hughes Bennett, 'Roy. Soc. Edinb.,' vol. xv., p. 284.

‡ Placed by Robin, 'Végétaux Parasites,' under the genus *Merismopœdia* of Meyen.

§ 'Edinb. Med. and Surg. Journ.,' 1842, p. 430.

For several days prior to death the bird had diarrhœa, but no other symptom of illness was noticed. In my examination of the body, the most notable feature was the presence of a layer or thin film of a pale greenish, mouldy, fungus-looking substance upon the pleural surface of the lungs. The underlying pleura was somewhat thickened and of a delicate roseate tint, apparently from injection of the minute capillary vessels. The lungs themselves exhibited spots of lobular pneumonia, that is, were irregularly hepatized. The intestines were nearly empty, the contents alone being bad-smelling fluid matter.

The cryptogamic growth as exposed on the pleural surface was little over 0·25 inch in diameter, and irregularly outlined. To the eye it looked like a mouldy patch, of a white colour with a greenish tinge. A hand lens showed a diminutive forest of suberect filamentous structures. A portion of the mould placed under the compound microscope, with a low power revealed constituents as subjoined. An abundance of linear filaments crossing and so interwoven with each other as to form a kind of open reticular meshwork. Some of these filaments were simple threads, but the greatest number had a denticulate character. Distributed amongst the capillary meshes were innumerable echinate circular cells apparently nucleated. These cells or spores here and there had attachments to the filaments, and in some cases at the margin of the field I observed a crucial or stellate figure produced by a spore being situate at the junction of two obliquely-placed threads. With a still higher magnifying power I could distinguish a set of oval or elliptical cells distinctly nucleated.

2. In the case of a European Rough-legged Buzzard, *Archibuteo lagopus*, Gmelin, whose pathological condition presented no other feature of importance, I likewise detected a fungous-like vegetable growth spread over the pleural membrane of the left lung. This bird, also an adult male, had been only about a month in the Gardens, and died on the 31st August, 1868, no symptom of disease meanwhile having been noticed.

In this case a greater area of the lung was covered by the mould, but which in colour and general appearance was a counterpart of that of the cockatoo.

3. The third example was that of a female Kittiwake Gull, *Rissa tridactyla*, Linn., which was received by the Zoological Society 31st December, 1868, and died on the 7th March, 1869. For a short time it was noticed to pine, and take its food badly. The body on death was in a poor condition, although its plumage seemed good. The viscera showed nothing specially worthy of note, excepting the right lung and its investing membrane. The latter had attached to it a pale greenish film of vegetable mucor similar to the two foregoing birds, and in this case, whether primarily or as a

secondary consequence, the pulmonary tissue itself had become somewhat atrophied. The vegetable parasite, moreover, seemed to have extended its areal distribution towards the ribs and breast-bone, the inner lining membrane (periosteum) of which, and even the bones themselves, were roughened.

The microscopic characters of the cryptogamic substance in this case agreed in most respects with that of No. 1; the echinate cells and filamentous threads being very distinct. Some other duties occupied my immediate attention at the time, so I could only satisfy myself of near identity with former cases, whilst my artist produced a faithful drawing of the vegetation *in situ*.

Probably one of the earliest, well-authenticated observations of vegetable growth within the body of a bird is that of the celebrated John Hunter, in his notes on the dissection of a Sea-gull (*Larus marinus*).* I quote it in full; for, from only being published of late years, though it is a century since his memorandum was written, Continental and other authorities do not allude to it. Moreover, it possesses another interest from the gull having been alive when undoubtedly the growth sprung up; for some writers have hinted that fungi may be developed subsequent to death, which this instance demonstrates to the contrary.

Hunter says:—"This bird I kept in the garden upon flesh for about a month; and about four or five days before I killed it, it was taken ill, so that it did not eat, but grew worse and worse till I killed it. On opening the abdomen I saw a number of white spots; some on the kidneys, some on the membranous partitions, and others upon the stomach. These I found to be chiefly mould: some parts of them were green, and had a down [Mucedo] upon them. This mould must have formed before death; and, although one can hardly believe it, yet so it must have been, for it was seen in less than twelve hours after death; and if a little could have been produced in that time, yet the whole could not, for some of it was more than a quarter of an inch thick. The membranous partitions were thick and inflamed, and I account for this mould in this way. We know that there is air within the cavity of the abdomen, which is taken in by the windpipe and goes through the lungs, from thence through the diaphragm into the abdomen. Now, if this air was at all confined, and did not get out for a supply of fresh to get in, it would certainly putrefy the juices that were thrown out by inflammation, and these juices might become mouldy before death. This is a hint that the air in an emphysema should be let out, and not allowed to become putrid."

Dr. Bennett's paper, already referred to, contains a succinct repertory of facts and literature concerning parasitical vegetables

* 'Essays and Observations,' edited by Prof. Owen, 1859, vol. ii., p. 329.

on living animals; but with respect to development within the thorax, &c., of birds, Dr. Charles Robin's* résumé has more completeness. The latter describes and refers to several kinds of fungi found by competent observers in situations nearly identical with those I have detailed. The following true, or supposed, distinct species of *Aspergillus* are denoted by him:—

A. candidus, Micheli.
A. glaucus, Fries.
A. nigrescens, Robin.
A. —— sp. ?, Müll. and Retz.—specimen from the Snowy Owl.
A. —— sp. ?, Mayer—specimen from the Jay.
A. —— sp. ?, Deslongchamps—specimen from the Eider Duck; Robin says probably *A. glaucus*.

According to M. Robin's determination of the epiphytes, six forms are noted; yet he only ventures to name three species, and one of these, his *Aspergillus nigrescens*, seems at present rather to be distrusted by our highest English authorities on Mycology. This reduces the really certified specific forms to two, *A. candidus* and *A. glaucus*.

As regards the cryptogamic vegetations which I have encountered, are they algæ or fungi? I put it so, as at the meeting of the British Association in Edinburgh, 1871, Mr. Cooke threw out the hint of the possibility of their belonging to the first-mentioned group. I had myself stated, and believe I have fair grounds for considering them to be members of the Order Mucedines, and species of the genus *Aspergillus*. The location, visceral cavity of a bird, is that wherein previous observers have registered their occurrence. The microscopical structure, I presume, endorses the opinion; for we have in the interwoven thread-like cells a representative of mycelium, and in the cells, echinulate spores identical with those of *Aspergillus glaucus*. On comparing my drawings of the microscopic structures with those of Müller and Retzius,† Deslongchamps,‡ Spring,§ and Robin,‖ I find them in most respects identical. A careful revision, moreover, of descriptions and original figures of *Aspergillus*, of several botanical writers,¶ establishes additional conviction.

* 'Hist. Nat. des Végétaux Parasites qui croissent sur L'Homme et sur les Animaux Vivant.' Paris, 1853, p. 545 *et seq*.

† 'Ueber Parasitiche Bildungen,' *l. c.* hereafter, Tab. viii., fig. 3.

‡ 'Note sur les mœurs du Canard Eider et sur des moisissures dévelopées pendant la vie, à la surface interne des poches aériennes d'un de ces animaux,' *l. c.* pl. xi., figs. 4, 5, 6.

§ 'Sur une Mucédinée dévelopée dans la poche aérienne abdominale d'une Pluvier Doré,' *l. c. infra*, Figs. 2 and 3 in plate accompanying paper.

‖ *L. c.* Champignons, pl. ii., fig. 4, *a*, *b*, *c*, *d*, *e*, *f*, et fig. 13.

¶ Sowerby, 'Eng. Fungi,' Lond., 1797–1803, pl. 378, figs. 5 to 9; Meyen, 'Pflanzenphysiologie,' figs. 20–22, pl. x., vol. iii.; Berkeley, 'Introd. to Cryptogamic Botany,' fig. 68, &c.

Including those already mentioned, the birds in which fungi have been discovered are as follows:—

European Rough-legged Buzzard, *Archibuteo lagopus*, Gmelin.
Red Buzzard, *Buteo rufus* (*Falco rufus*)? *
Snowy Owl (*Strix*), *Nyctea nivea*, Daud.†
The Jay, *Corvus glandarius*, Temm.‡
Common Bullfinch, *Pyrrhula vulgaris*, Linn.§
White-crested Cockatoo, *Cacatua cristata*, Linn.
Parakeet, —— Gn., —— sp. ? ||
The Pigeon, *Columba*.||
Domestic Fowl, *Gallus*.||
Common Pheasant, *Phasianus colchicus*, Linn.¶
Golden Plover, *Charadrius pluvialis*, Linn.**
Ruddy Flamingo, *Phœnicopterus ruber*, Linn.††
Stork, *Ciconia*, —— sp. ? ‡‡
Common Swan, *Anas olor*, Gmelin.§§
Eider Duck, *Somateria* (*Anas*) *mollisima*, Linn.||||
Kittiwake Gull, *Rissa tridactyla*, Linn.
Great Black-backed Gull, *Larus marinus*, Linn.

From the foregoing it results that seventeen kinds of birds are liable to attacks of fungi, either the precursor and causing, or a sequence of disease terminating fatally. Different avine groups are each represented, Raptores, Passeres, Scansores, Gallinacea, Grallatores, and Natatores, so that we may infer the entire class is predisposed under given but as yet unknown conditions.

I think it will hardly be advanced by anyone that these cases support the idea of spontaneous derivation, seeing that spores might be carried by the lungs close to the seat of their germination; penetration of the tissue and development thereafter following in quick succession. The route of entrance of the germs by the respiratory organs is more than a likely one; for, as Berkeley¶¶ very justly says, "the spores of our common mould float about everywhere; and as they grow with great rapidity, they are able to establish themselves on any surface where the secretion is not sufficiently active or healthy to throw off the intruder. Where the spores are very abundant, they may sometimes, like other minute bodies, obstruct the minute cells of the lungs."

The penetration of the spores through the pulmonary and pleural tissues is a matter of less wonder,*** judging from what is

* Observed by Herr Dubois, quoted by Johannes Müller, *infra*.
† Müller and Retzius, Müller's 'Archiv,' 1842, p. 203, Taf. viii., ix.
‡ Mayer, in Meckel's 'Archiv,' 1815, p. 310.
§ MM. Rayer et Montague, 'Journal Institut.,' 1842, p. 270.
|| E. Rousseau et Serrurier, 'Compte Rendu,' 1841, xii., p. 18.
¶ Robin, 'Soc. de Biolog.,' Juin 20, 1848, and 'Vég. Par.,' p. 258.
** Spring, 'Bull. l'Acad. Sci. Belg.,' 1848, t. xv., p. 486.
†† Owen, P.Z.S., 1832, p. 141.
‡‡ Heusinger, 'Bericht v. d. Kong. Zoot. Anst. z. Wurzburg,' 1826, p. 26.
§§ Jaeger, Meckel's 'Archiv,' 1816, p. 354.
|||| E. Deslongchamps, 'Ann. des Sciences Nat.,' 1841, p. 371, pl. 11.
¶¶ 'Outlines of British Fungology,' p. 69.
*** *Vide* Robin's remarks, *l. c.*, "Pénétration des spores dans les cavities closes," p. 283.

known respecting fungoid development in insects, wood and other hard substances.

I may here note that Johannes Müller thought he had discovered another cryptogamic form of the genus *Peziza* in the cases which came under his observation. This moot point has been discussed by M. Spring and Professor Robin. From what I have seen in the three birds, Robin's demonstration of the supposed *Peziza* being only circular spots of basement or false membrane, upon which matrix the mycelium of *Aspergillus* is borne, appears the correct view. In my own case I easily recognized a pellicle of fibrinous exudation with the characters attributed and carefully figured by Deslongchamps.

The interest at present attached to cryptogamic developments as a source of disease, has given rise to much discussion on the Continent; and in this country, along with the theory of spontaneous generation, almost a literature of its own.*

It is not my intention to enlarge on the pathological aspect of the present cases, but the impression left on my mind is that to the development and fructification of the Mucedines the death of the birds may be attributed, inasmuch as the specific lesion was due to their presence. But I couple as a proviso my impression that an antecedent state of the system admitted of the development of the fungus.

To those who doubt the capabilities of fungoid growths in active vital parts, solid and otherwise, I would advise them carefully to study Robin's chapter v. (*l. c.*, p. 278), "Action Exercicée par le Végétal sur l'Animal;" to be convinced that the so-called Mycetoma (Madura, or Fungus-foot of India) is quite within the range of possibility, irrespective of primary putrefactive process.

Dr. Leidy, no mean observer, on discovering many new epiphytic forms, says,† with justice,—that we have entophyta in luxurious growth within living animals, without affecting their health; yet at the same time he admits that there are cryptogamia capable of producing and transmitting disease, as in the case of muscaridine, &c.

P.S.—Prof. W. T. Thistleton Dyer has lately drawn my attention to the Reports on Botany, Ray Soc., 1846, p. 424, where it is mentioned a Scaup Duck, *Fuligula marila*, Linn., had mucor or blue mould in the lungs and membranes.‡ Other cases I have quoted are also referred to in the Report (*l. c.*).

* I need here but refer to Prof. Huxley's Address, Brit. Assoc., Liverpool, 1869; Prof. Lionel Beale's 'Protoplasm,' his 'Disease Germs,' 1870; and Dr. Bastian's 'Modes of Origin of Lowest Organisms,' and 'The Beginnings of Life,' 1871, as typical of what I mean.

† 'Proc. Philad. Acad.,' 1848–9, vol. iv., pp. 228, 229.

‡ Yarrell, in 'Ann. Nat. Hist.,' vol. ix., p. 131.

II.—*Some Remarks on the Finer Nerves of the Cornea.*

By Dr. E. Klein.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

Plates XIII. and XIV.

It is not long since that I had the opportunity to give a description of the nerves of the cornea.* If I undertake to speak again on the same subject, there are two reasons: first, I am able by using a new method—though only a modification of the common method of staining with chloride of gold—to demonstrate very easily, and with great certainty, even the finest nerve fibres.

The great advantage which this method has over the one I have previously described is, that the preparations show most abundantly the finer nerve fibres, and that the latter come into view very sharply defined; accordingly they can be examined with the greatest ease even under a lower power, as 250 to 300, and with a small amount of light.

EXPLANATION OF PLATES XIII. AND XIV.

Nerves of the Cornea.

Fig. 1.—Horizontal preparation of nerves of the proper substance of the rabbit's cornea. *a*, coarser non-medullated nerve trunk. *b*, finer non-medullated nerve fibres. × 300.

„ 2.—Horizontal preparation of the sub-epithelial nerves of the guinea-pig's cornea. *a*, coarser non-medullated nerve trunks of the sub-epithelial plexus. *b*, finer non-medullated nerve fibres. *c*, finest non-medullated nerve fibres of the sub-epithelial network. × 300.

„ 3.—Oblique section through the deeper epithelium of the anterior surface of the rabbit's cornea, and the superficial layers of the proper substance. *a*, coarser non-medullated nerve trunk. *b*, brushes of fine non-medullated nerve fibres. *c*, fine non-medullated nerve fibres of the deep intra-epithelial network. *d*, deeper epithelial cells. × 300.

„ 4.—Horizontal preparation of the rabbit's cornea. *a*, coarser non-medullated nerve trunks. *b*, fine non-medullated nerve fibres. *c*, brushes of finest sub-epithelial nerve fibres. × 300.

„ 5.—Horizontal preparation of the guinea-pig's cornea. Superficial intra-epithelial network of fine non-medullated nerve fibres. × 300.

„ 6.—Horizontal preparation of the rabbit's cornea. Superficial intra-epithelial network of fine non-medullated nerve fibres. × 300.

„ 7.—Horizontal preparation of the frog's cornea. *a*, non-medullated nerves of the coarser plexus of the proper substance of the cornea (nerves of the first order). *b*, *c*, and *d*, fine non-medullated nerve fibres (nerves of the second and third order). × 300.

„ 8.—Horizontal preparation of the frog's cornea. *a*, non-medullated nerve trunk of the first order. *b*, non-medullated nerve fibres of the second order. *c*, non-medullated nerve fibres of the third order. *d*, non-medullated nerve fibres of the fourth order. *e*, corneal corpuscles. *f*, nuclei of the same. Hartnack Immersion, Nov. 10. Copied from the 'Quarterly Microscopical Journal,' October, 1871.

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* 'Quarterly Microscopical Journal,' October, 1871.

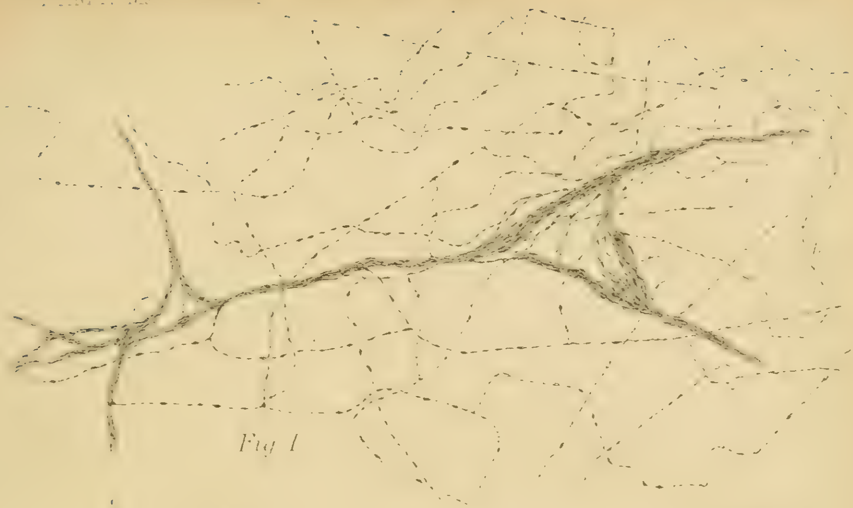

Fig 1

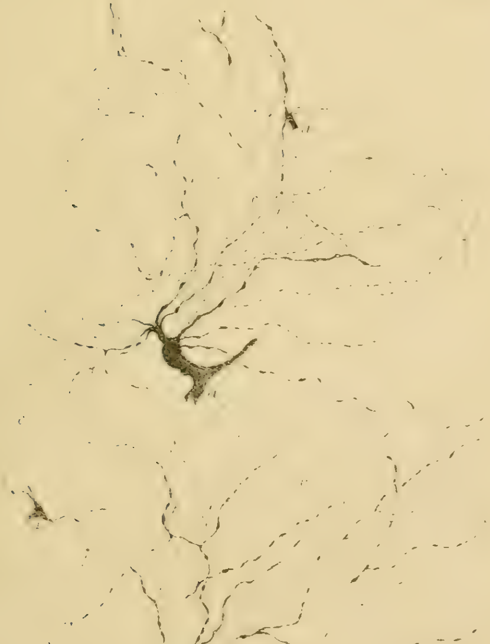

Fig 2

Fig 3

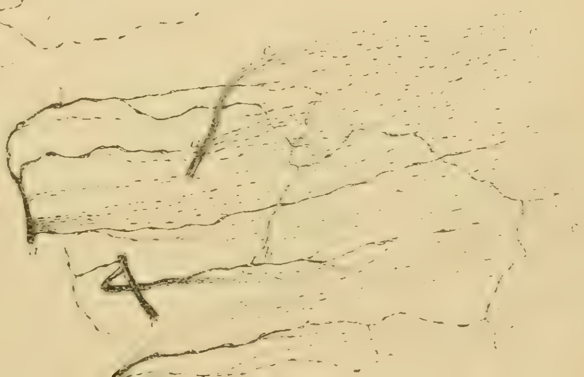

Fig 4

This method is as follows:—The cornea of a rabbit or a guinea-pig is cut out (it is of no consequence if this is done a quarter or half an hour after death) and placed in a half per cent. solution of chloride of gold. The cornea of the rabbit remains in the solution for from one hour and a half to two hours; that of the guinea-pig for from one hour to one hour and a quarter. After that the cornea is washed in distilled water, and exposed to the light in distilled water for from twenty-four to thirty-six hours (the water being changed twice, or oftener); after this time has elapsed the cornea is transferred into a mixture of one part pure glycerine and two parts distilled water, where it remains for two or three days. Up to this time the cornea has not assumed a darker colour than ash grey, perhaps having in a small degree a violet tint; at all events, the whole of the cornea is transparent. The cornea is then brushed over on its anterior surface under water with a fine camel's hair brush very gently, so as to remove the precipitates of the gold salt.

It is quite sufficient to brush it only once very gently and delicately, so as to clear the surface, even if not over its whole extent, still over the greater portion of it. No great practice is required to obtain from such a cornea, with a sharp razor, thin horizontal and oblique sections, by placing it simply with its concave surface on the volar side of the last phalanx of the first finger and fixing its scleral border with the thumb of the same hand. The sections are examined in glycerine.

The sections which are made in an oblique direction through the anterior epithelium and the superficial portions of the proper substance of the cornea are as valuable as the horizontal sections. The latter are necessary for the examination of the sub-epithelial nerve fibres as well as for the intra-epithelial nerve fibres, both separately, although in a binocular microscope there is no difficulty whatever in seeing how the latter spring off from the former.

The examination of the oblique sections proves this thoroughly.

I have prepared, by the above-mentioned method, sixteen corneæ in a relatively short time, and not one of these has failed. Of course, not all of them were in all parts equally perfect.

I do not wish to give any long description of the relation of the sub-epithelial plexus of the coarse non-medullated nerve fibres, or, more properly speaking, of the sub-epithelial plexus of the nerve trunks, nor of the sub-epithelial network of fine fibres, nor of the immense number of very fine fibrillæ which spring off, in bundles like a brush, from several branches of the sub-epithelial trunks, which branches run close to the under surface of the anterior epithelium. I wish only to mention shortly the fine nerve fibrillæ which enter the epithelium, and there form a deep and a superficial intra-epithelial network. I refer simply to the description I have already given, and to the adjoining figures.

In all the preparations obtained by the method above mentioned, the nerve fibres, up to their finest ramifications, appear as sharp, outlined structures, of a dark colour. No other element is coloured besides the nerve fibres. Of the cellular structures of the proper tissues of the cornea nothing is to be seen. Although the epithelium is also not, at least not for a pretty long time, coloured, still its every cell is very distinctly to be seen.

In all these preparations the fact which can be deduced with certainty is, that only the trunks of the sub-epithelial plexus, which as well as the trunks which are situated deeper in the substance of the cornea are only bundles of fine nerve fibrillæ, are provided with a nucleated sheath. In all other nerve fibres, *i. e.* the nerve fibres in the proper substance of the cornea (see Fig. 1), the fine nerve fibres of the sub-epithelial network, as well as the very delicate fibrillæ, which close under the epithelium spring off like a brush (see Figs. 2, 3, and 4), and, finally, the nerve fibrillæ of the intra-epithelial networks,—in all these, I say, there is nothing like nucleus to be found.

As regards the frog's cornea, I also refer to the description and illustrations I have given in my first memoir in the 'Quarterly Journal of Microscopical Science.' Here also only the nerves of the coarser plexus are provided with a nucleated sheath. The nerve fibres, which I have described as nerve fibres of the second order, and which run very often in a winding course, sometimes also rectilinear, exhibit nuclei only in very few places. The nerve fibres of the third order, which run mostly rectilinear, and which are distinguished like the fine nerve fibres in the rabbit's cornea by more or less regular varicosities, no longer exhibit nuclei. These nerve fibres of the third order, which remain for a long distance undivided, join by rectangular fibres, so as to form a trellis-work, the meshes of which are relatively broad; in the same manner also the nerve fibrillæ of the fourth order, which join to a dense network upon the corneal corpuscles themselves, have no nuclei.

We find therefore also in the cornea of the frog, that the finer nerve fibres do not possess any nucleus. The special mode in which the finer nerve fibres of the corneal substance run is not a peculiar one, if we consider that the arrangement of the bundles of the corneal tissue is a regular one, and that the nerve fibres can only run between these bundles.

Whether the fine fibres described in the rabbit's and the frog's cornea are nerve fibres at all cannot be the subject of any doubt, if we admit, as regards the microscope, that our mental eye is not able to deny what our normal and healthy bodily eye sees as an atomical continuity.

The second reason why I take the opportunity to speak on the

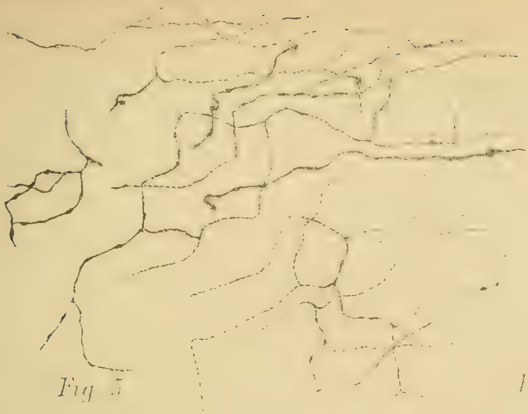

Fig 5

Fig 6

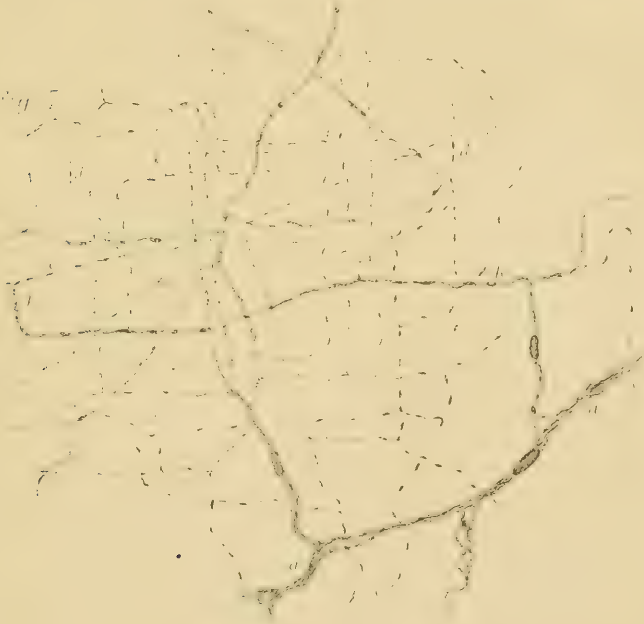

Fig 7

Fig 8

nerves of the cornea is that some remarks have been made by Dr. Beale before this Society, on finer non-medullated nerve fibres; how far these are right I shall be able to show. First of all, I find in his memoir in the 'Monthly Microscopical Journal' for January, 1872, page 4, the following sentences:—"My own conclusions on the ultimate distribution of nerve fibres were formed several years ago, at a time when terminal nerve-networks were denied in Germany, and it was supposed that only in a few exceptional cases did the axis cylinder of a nerve extend beyond the white substance. Not only are my networks of pale nucleated nerve fibres now accepted, but it is maintained that much finer networks of nerve fibres ramifying upon and amongst epithelial cells and other elementary parts, and even upon an individual mass of bioplasm (nucleus) have been demonstrated. At present, however, I cannot regard the observations upon which it is thought to establish this view more conclusive than those which a few years since led many to the conclusion that the axis cylinder sprang from the nucleus or nucleolus of the central nerve cell."

The first descriptions of a terminal network of non-medullated pale nerve fibres are due to Remak. Remak* described networks of non-medullated nerve fibres, which surround the striped muscular fibres, and which Remak regarded as terminal networks.

As regards the striped muscles, these assertions of Remak have been confirmed by Schaafhausen,† by the aid of carmine and staining. Next comes the assertion about terminal networks of non-medullated nerve fibres of common sensation in the skin and the mucous membranes. Kölliker, in 1856, described a network of pale nucleated non-medullated nerve fibres in the skin of many mammals.‡ Kölliker did not regard this network as a terminal one.

About the same time, Axmann describes network of non-medullated nerve fibres in the skin of the frog. Further, I have to state that His described in the substance of the cornea of mammals a terminal network of pale non-medullated nerve fibres.§

Then I have to notice the assertions of Meissner of a terminal plexus of non-medullated nerve fibres in the submucosa of the intestine.‖

A short time after that Billroth described in different places of the mucous membrane of the digestive tract terminal networks of pale non-medullated nucleated nerve fibres.¶ Next comes the confirmation of His's terminal network of non-medullated fibres in the

* 'Archiv für Anatomie der Physiologie,' 1843, p. 189.

† 'Amtl. Berichte der Naturforscher.' Vers. zu Bonn, 1859, p. 193.

‡ 'Zeitschrift für wissenschaftl. Zoologie,' Bd. VIII.

§ 'Beiträge zur normalen und pathologischen Histologie der Cornea.' Basel, 1856.

‖ 'Zeitschrift für rationelle Medizin,' VIII., p. 364. 1857.

¶ Müller's 'Archiv,' 1858, p. 148.

cornea of mammals by Arnold,* who, in 1862,† described also in the conjunctiva of several mammals terminal networks of non-medullated nerve fibres. Saemisch then amplified our knowledge about the terminal network of non-medullated nerve fibres in the cornea of mammals.‡

Further we have to notice, that as regards the smooth muscular fibres Kölliker made assertions about non-medullated nerve fibres in the smooth muscular tissue of the urinary bladder of the frog; § according to Kölliker the non-medullated nerve fibres form on some places a network both in the muscular tissue as well as on very small arteries and veins.

Then Auerbach described in the smooth muscular wall of the intestine non-medullated nerve fibres forming plexus.‖ After that Klebs described networks of pale non-medullated nerve fibres in smooth muscular tissue,¶ and still later ** Arnold and His described terminal networks of pale nucleated nerve fibres in the muscular coat of large vessels.

Beale's first assertion about a terminal network of non-medullated nerve fibres refers to striped muscles, 'Philosophical Transactions,' 1860, and therefore seventeen years after Remak and one year after Shaafhausen. Beale has been supported by Kölliker (1862).

About the capillary vessels Beale was the first and the only observer who described non-medullated nerve fibres, which accompany the vessel, bend round it, and join by lateral branches ;†† it is further due to the truth to say that Beale is the only one who described (and this was pointed out long before also by Klebs, Virchow's 'Archiv,' 1865, Bd. 32, p. 185) a real network of non-medullated nucleated nerve fibres in the muscular coat of smaller arteries.‡‡ Beale described (and illustrated by most beautiful illustrations) that in the papillæ fungiform as well as in the papillæ filiform the non-medullated nerve fibres form a plexus.§§ In the papillæ fungiform Beale is not quite sure whether the fine nerve fibres terminate below the epithelium, or between its cells.

We see therefore that the second stage of the doctrine of the nerve termination (the first stage being this, that the medullated nerve fibres bend round and back in loops), that is, the doctrine of

* Heidelberg, 1860.

† Virchow's 'Archiv,' Bd. 24.

‡ 'Beiträge zur Anatomie des Auges.' Leipzig, 1862.

§ 'Verhandlungen der phisik.—medicin. Gesellschaft zu Wurzburg,' 1862. 1 Heft. January. p. 1.

‖ 'Uëber einer plexus myentericus.' Breslau, 1862, p. 12.

¶ 'Centralblatt,' 1868, No. 36.

** Virchow's 'Archiv,' Bd. 28.

†† 'Philosoph. Transact.,' 1863, pl. lx., figs. 44 and 47; and 1865, pl. xxii., fig. 15.

‡‡ 'Philosoph. Transact.,' 1863, p. 563, pl. xl., figs. 45 and 46.

§§ Ibid., 1865.

terminal networks of non-medullated nerve fibres, is, except the capillaries and the muscular coat of small arteries, neither in striped muscles, nor in the cornea, nor in the skin, nor mucous membranes of different kinds, due to the source which Beale asserts in the above-mentioned paper,* although he has advanced this doctrine in many respects in a remarkable degree.

On the other hand, further investigations show, that THIS doctrine of terminal networks has been shaken in many respects. I wish to call attention to the fact that in striped muscles the inter-muscular network was not accepted as a termination, neither at the time of Shaafhausen, nor of Beale, nor up to the present time; that is proved by the great number of excellent researches on nerve terminations in striped muscle in the most various classes of animals, by Doyère, Meissner, Wedl, Walther, Munck, Kölliker, Rouget, Krause, Engelmann, Kühne, Cohnheim, &c., &c. By these observers it has been shown, that the nerve fibres do not terminate as inter-muscular, but that there exist also intra-muscular fibres, whose exact mode of termination in various cases has not yet been fixed. Of course if Beale, even at present, adheres to an inter-muscular termination, I am quite able to understand his inability to proceed further, because by his method of preparation, only inter-muscular nerves can be shown.

A good fresh preparation of isolated muscular fibres, which Dr. Beale does not appear to have ever examined, will show him that even in the frog, not to mention the muscles of insects and reptiles, there are intra-muscular nerves.

In smooth muscular tissue it has been shown further, that it is possible to trace fine non-medullated nerve fibres beyond this known plexus.†

As regards the nerves of common sensation, in the cornea substance, first of all, Kühne ‡ has shown that beyond the supposed terminal network there are to be traced very fine nerve fibrils, which are, according to Kühne, in communication with the corneal corpuscles. Then it has been shown by Floyer,§ that from the sub-epithelial network of fine non-medullated nerve fibres spring off fine fibrils, which enter between the cells of the anterior epithelium. Cohnheim,|| Kölliker,¶ and Engelmann** have extended this assertion to a very remarkable extent.

As regards the skin, it has been shown that beyond the plexus

* 'Mon. Micr. Jour.,' January, 1872.

† Klebs, *l. c.*, Frankenhäuser, 'Die Nerven der Gebärm und ihre Endigung in den glatten Muskelfasern,' 1867; Arnold, cap. iv. Stricker's 'Histologie.'

‡ Kühne, 'Gazette Hebdom.,' tome ix., Paris, 1862; und 'Untersuch., über das Protoplasma,' Leipzig, 1864.

§ Floyer, 'Archiv für Anatom. und Phisiolog.,' 1866. Heft II., p. 180.

|| Cohnheim, 'Centralblatt,' May, 1866; No. 26, Virchow's 'Archiv,' 38 Bd.

¶ Kölliker, 'Sitzungsber der phisik—medic. Gesellsch.' zu Wurzburg, Juni, 1866.

** Engelmann, 'Die Hornhaut des Auges.' Leipzig, 1867.

of non-medullated nerve fibres there are to be traced fine non-medullated nerve fibres amongst the epithelial cells.*

For the mucous membranes it has been shown in the same manner, that beyond the plexus of non-medullated nerve fibres belonging to the proper mucous membrane are to be traced fine non-medullated nerve fibres amongst the epithelium.†

I must admit that I agree with Beale in not defending a free termination for nerves of common sensation. Because I have been able to convince myself, amongst other points (nerves of the tad-pole's tail, nerves of the serous membranes, nerves of the blood-vessels): 1. That in the substance of the cornea I could trace beyond the known fine non-medullated nerve fibres, still finer fibrils than those which have been supposed to be in connection with the corneal corpuscles, and which finer fibrils still join to form a network.

2. That the non-medullated nerve fibres which enter the anterior epithelium of the cornea and of the nictitating membrane of the frog, do not terminate in free extremities, but join in a network.

3. That the observations made upon the epithelial nerves of the mucous membrane of the vagina (Chrschtschonovitsch) and mouth (Elin) made under my direction, do not show free terminations.

Beale does not admit that there exist fine nerve fibres which spring off from the already well-known plexus in the cornea, mucous membrane, skin, &c., and enter the epithelium—and regards the above-mentioned plexuses (which as I have pointed out are not to be called *his* plexuses) as terminal. For the nerves of the cornea especially, I am able to demonstrate as clearly as possible that his views are entirely erroneous. What Beale calls finest ultimate nucleated nerve fibres are only compound fibres; of the really finest fibres, which have nothing to do with nuclei, Beale has seen absolutely nothing. I must most decidedly call that view erroneous, according to which the presence of a nucleus is in any way decisive anatomically of fine non-medullated nerve fibres, whether in cornea, skin, mucous membrane, or elsewhere. So long as a non-medullated nerve fibre has a sheath, a nucleus can also be found upon it; but when the fibre has divided into single fibrillæ, or into small bundles of these, the sheath (and consequently the nucleus) is no longer to be found. The fibrillæ which come off from a non-medullated nerve fibre, either singly or in small bundles, have absolutely nothing to do with any nucleus whatever. Beale apparently no longer adheres to his former view‡ that "all fibres which can be followed for a considerable distance, which refract like true nerve fibres, and exhibit an appearance more or less

* Langerhans, Virchow's 'Archiv,' Bd. 44; Podkopaeff, Max Schultze's 'Archiv,' Bd. V.; Eberth, Max Schultze's 'Archiv,' Bd. VI.

† Chrschtschonovitsch, 'Sitzunsber. der Akad. der Wissen in Wien,' 63 Bd., Februarheft, 1871; Elin, Max Schultze's 'Archiv,' Bd. VII., 4 Heft.

‡ 'Philosoph. Transact.,' 1863, p. 565.

granular . . . . . . must clearly be pronounced nerves." And,* "In all cases, as far as I can ascertain, the ultimate terminal fibres are pale and granular, exhibiting nuclei at varying intervals." For in a recently-published paper,† which is stated to have been read before this Society on 6th December, 1871, he admits that his delicate nerve fibres are compound, that they are fibrillar, and that the nuclei are usually "situated more or less on one side." We see therefore that he here withdraws from his previously-expressed opinions, although he uses the same Plate ‡ to illustrate his new views, which had already done service in illustrating the old ones.§

He partly abandons also his former views, and of this I am very glad, as he admits that ‖ "it is quite possible that there may be still finer fibres." This encourages me to hope that by the use of the above-described method of staining with chloride of gold, he will be led entirely to abandon his views about the fine nerve fibres of the sub-epithelial network of the cornea of mammals, about fine nerve fibres among the epithelial cells, and about the presence of nuclei (bioplasts, or germinal matter). In this last paper of Beale's, where he now admits that the delicate nucleated nerve fibres are still compound and fibrillar, and by doing so only confirms what was already known years ago, even from the study of fresh preparations alone, he asserts that the fibrillar structure is to be seen in his preparations, and not in gold or osmium preparations. This I must call a gross error, because on the one hand, as I have pointed out, the very same preparations of his, which, until 1868, had only a granular appearance, in February, 1872, appeared fibrillar. On the other hand, it has been shown in several cases that, both in gold and in osmium preparations, fine non-medullated nerve fibres, which spring off from the plexus of nucleated fibres, still show a fibrillar structure.

I agree with Beale up to a certain point when he says that in general with his method (carmine and glycerine) he is not able to demonstrate more than the plexus of nucleated non-medullated nerve fibres, although I have been in the agreeable position of ascertaining on one of his own preparations of the cornea, that his view that the nucleated fibres are the finest and ultimate, by no means agrees with the facts.

By the help of the gold method we are able to show with certainty that all hypothetical objections, based upon the probability or improbability of the existence of nerve fibres among epithelial cells, vanish completely, as they are at once exploded by simple observation of the facts.

* 'How to Work with the Micr.,' 4th edit., 1868, p. 332.
† 'Mon. Micr. Jour.,' Feb., 1872, p. 47.
‡ Pl. VI., 'Mon. Micr. Jour.,' Feb.
§ 'Philosoph. Transact.,' 1863, pl. xl., fig. 43. ‖ 'Mon. Micr. Jour.,' p. 46.

As regards Dr. Beale's doubting or denying all that he is not himself able to show, I venture to recommend to his attention the following sentences:—. . . . "seems to conclude, in too many cases, that what he has failed to see does not exist." * Further, "It is possible that for many years to come some observers will persist in terming everything in which they fail to demonstrate distinct structure, connective tissue."† Further, "It is remarkable how positively many authorities deny the existence of structures which they have failed to demonstrate."‡ And, finally, "Moreover, many observers seem to have determined in their own minds what appearance a fibre should present to be entitled to be regarded as a nerve, and then they arbitrarily assert that a fibre which does not present these characters cannot be nervous; and even if it be continuous with an undoubted nerve fibre, it is put down as connective tissue."§

* Beale, 'Philosoph. Transact.,' 1863, p. 563.
† Ib., *loc. cit.*, p. 567. ‡ Ib., *loc. cit.*, p. 567. § Ib., *loc. cit.*, p. 568.

III.—*Note on the Resolution of Amphipleura pellucida by certain Objectives made by R. and J. Beck, and by William Wales.*

By Dr. J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

In the 'Monthly Microscopical Journal' for Sept., 1871, p. 150, I recorded the resolution of this difficult diatom by an immersion $\frac{1}{5}$th made by Mr. Tolles, of Boston, and sent photographs exhibiting the performance, to the Editor, as well as to the Royal Microscopical Society.

The same desire to do full justice to all honest efforts to improve the microscope, which led me to make that brief publication, induces me now to give like publicity to the performance of the new immersion $\frac{1}{10}$th (so-called) of Messrs. R. and J. Beck.

In the first instance, I thought it worthy of note that, by excellent workmanship and high angle, a lens of such comparatively low magnifying power could achieve so great a feat of resolution. In the second instance, the objective having more than double the magnifying power, the result is only surprising on account of the extremely low price at which the makers have undertaken to furnish it.

Mr. Jos. Beck exhibited during his visit to America an immersion $\frac{1}{10}$th of excellent quality. One was ordered for the Army Medical Museum, which has recently come to hand, and it is of its performance I now speak. These glasses are remarkably cheap as compared with any similar ones hitherto brought to my notice.

The objective received at the Museum is characterized by great flatness of field, freedom from colour, and depth of penetration. Its performance on *Amphipleura pellucida* may be fairly judged from the accompanying photograph,* which is magnified 1025 diameters. On the Nobert's nineteen-band plate I get it satisfactorily through the sixteenth band. As to its magnifying power and equivalent focal length, I obtained the following results:—With 50 inches distance from micrometer to screen the magnifying power was 570 diameters at uncovered, and 630 at full correction for cover. Taking uncovered as a standard, therefore, the focal length should be rated at about $\frac{1}{11}$th inch. The angle of aperture at covered is about 160°.

A few days after taking the photograph of *Amphipleura pellucida* above referred to, I made another photograph of the same diatom with an objective made for the Museum two years ago by Mr. Wm. Wales, of Fort Lee, New Jersey. This is the objective

* Photographs were received from the author, and fully bear out what he states.—Ed. 'M. M. J.'

to which I referred in the 'Monthly Microscopical Journal' for December, 1869, p. 292, as resolving the seventeenth band of the Nobert's nineteen-band plate. The picture of *Amphipleura* obtained exceeds all I have been able to do on this diatom with any objective except the immersion $\frac{1}{16}$th (so-called) of Messrs. Powell and Lealand. I forward a print herewith showing a pair of frustules, both handsomely resolved from end to end, with about 1500 diameters. For comparison I send a picture of the same frustules taken with 1650 diameters by the Powell and Lealand $\frac{1}{16}$th.

The Wales's objective was invoiced to the Museum as a $\frac{1}{30}$th. A determination of its magnifying power, however, shows that at uncovered it magnifies rather less, at covered rather more, than the Powell and Lealand objective. I get by actual measurement the following results:—At 50 inches distance from micrometer to screen this objective magnifies 890 diameters uncovered, 1250 covered. Its equivalent focal length at uncovered is therefore $\frac{1}{18}$th very nearly.

In conclusion, I may add a few words with regard to the use of *Amphipleura pellucida* as a test-object. This diatom is a useful and valuable test for immersion objectives of $\frac{1}{8}$th inch focal length or less. Lower powers can only hope to resolve it if possessed of excessive angular aperture. When, however, it is desired to discriminate between small differences in the excellence of objectives intended for the most exquisite resolution, a more subtle test is required, and this will be found in the nineteen-band plate of Nobert, by those who take proper precautions in its use. Those, however, who believe they have secured resolution whenever they see lines in the higher bands of the plate, without duly considering their number, must not be surprised if objectives they have accepted as resolving the ultimate bands of the plate fail to show the striæ on even the coarsest frustules of the *Amphipleura pellucida*.

IV.—*Stephenson's Erecting Binocular.*

By J. W. Stephenson, F.R.A.S., Treasurer R.M.S., and Actuary to the Equitable Assurance Society.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

Plate XV. (Lower part).

In concluding the description of an erecting binocular microscope, which I had the honour of bringing before the Society in the year 1870, I remarked that possibly improvements in the arrangement or angles of the prisms might subsequently be devised.

It soon became evident that this was so, and that the mass of glass used in the lower prisms, by which the light on leaving the objective is divided, and the image laterally inverted, might be considerably diminished by bringing the latter closer to the back combination of the object-glass, and also that by this means higher powers than those which I had hitherto employed might be used; at the same time, for reasons to be hereafter explained, it appeared desirable to alter the angles at which the bodies are inclined to the perpendicular, from 75° to 66½°.

Under the present arrangement, two truncated rectangular prisms are still employed for dividing the light and inverting the image, but their dimensions are so far reduced that they are capable of being inserted into the object-glass itself (Plate XV., lower part); this is accomplished by placing them in a small brass tube, which is fixed in and projects beyond the nozzle of the instrument, but without in any way affecting the screw; they are each ·68 of an inch in length, ·412 of an inch in width, and ·2 of an inch in thickness, and are inclined to each other at an angle of 4¾°; this makes the angle between the bodies 9½°, and the imaginary point towards which the eyes converge nearly 15 inches.

This angle was selected as being generally the most suitable, bearing in mind the desirability of maintaining the ordinary length of body, and of giving an amount of convergence which should relieve the eyes of the strain not unfrequently experienced in the use of a binocular. In some instances where the eyes of observers are either closer together or wider apart than usual, it may be desirable to deviate from this angle in either direction, to meet the requirements of a particular case.

It is a matter of vital importance that the prisms be not too

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EXPLANATION OF PLATE XV. (Lower part).

The Plate represents an ideal section of the front of the microscope, removing half of the binocular prisms, the objective, and their fittings. The objective is shaded with oblique lines.

long in proportion to their thickness, for if this be so, the central rays, being reflected immediately on entering near the lower edges of the prisms, will strike the opposite sides and never emerge.

It is needless to say that if the central rays are thus lost, good definition is impossible.

I have given the length and thickness of my prisms as ·68 and ·2, but it is certainly undesirable that the length should exceed three and a half times the thickness.

The very unsatisfactory results produced by the Nicol's prism, when used as an analyzer in a binocular, was the primary cause of the alteration in the angle at which the bodies are inclined to the perpendicular.

It was evident that the analyzing prism might be entirely dispensed with by substituting for the upper reflecting prisms a highly-polished and properly-worked plate of black glass, so placed that the light should be reflected from its surface at the polarizing angle of $56\frac{3}{4}°$, and that this change could be accomplished without in any way affecting the character of the instrument as an erecting binocular by altering the angle at which the bodies were inclined from 75° to $66\frac{1}{2}°$. The box containing the upper prisms of the ordinary arrangement is withdrawn when polarized light is used, and the analyzing plate substituted; the result is improved definition and more light than with the ordinary Nicol.

I have spoken of the box containing the upper *prisms*, but in the original description of the instrument there was one prism only; that prism I have now divided, and the two thus formed are cemented together at such an angle that the light enters and emerges at right angles to the surfaces, care of course being taken that the reflecting surfaces remain in the same plane.

Still anxious further to improve the arrangement, I have caused a plane mirror to be prepared by Mr. Browning, and silvered by the beautiful process employed by him in the manufacture of reflecting astronomical telescopes. This, like the analyzing plate of black glass, can be immediately substituted for the upper prisms if desired. By its use we obviously get rid of the whole of the glass, and two surfaces; but, on the other hand, light is not totally reflected from silver, as it is by interior reflexion, and metal will tarnish.

I have thought it desirable to bring these alterations before the notice of the Fellows, as by the former less than one-fifth of the original quantity of glass is employed in the manufacture of the upper prisms, and powers up to an $\frac{1}{8}$th of an inch can be used with facility; whilst by the latter, in getting rid of the Nicol's analyzing prism we secure perfect illumination of both fields, better definition and more light.

The development of Actinophrys.

Stephenson's erecting binocular arrangement.

V.—*On a presumed phase of Actinophryan Life.*

By J. G. Tatem.

Plate XV. (Upper part).

Actinophrys sol is so often seen in the conjugated condition, and the progress of coalescence between two or more individuals so frequently watched, that I need not refer to the mode of its accomplishment. Nor is it necessary that I should remind the reader that much difference of opinion formerly existed as to the nature of the process; authorities having been equally divided on the subject, some regarding it as a Reproductive—a copulative act, others contending that it is due to accidental contact and partial fission only. Later observers have, however, determined the question by demonstrating the formation and expulsion of swarms of "embryonic germs" as the consequence of the union.* Whether or not these germs, so copiously diffused into the surrounding water, can ever be kept under observation, or be artificially maintained in the conditions favourable to and for the length of time necessary for their development, from embryo to maturity, and that, too, through many possible mutations of form, is altogether problematical. But may we not hope that, among the multitude of minute and scarcely-recognized monadiform beings, one or more may be detected which we may reasonably connect with the *Actinophryan* germ in its progress towards adolescence. I think so; and to one such I would wish to draw your attention.

In water in which the *Actinophrys sol* is more or less abundant, especially in the later months of autumn, certain minute organisms about $\frac{1}{1000}$ to $\frac{1}{900}$ are usually found. They are mostly of oblong but inconstant form, transparent, containing a few rather large granules having a greenish refraction, and armed posteriorly with soft-looking spines (pseudopoda), variable in length and number in individuals, and in the same individual also, from time to time (Plate XV., upper part, Figs. 1–3). Their motion is that forward oscillatory one which is effected by the vibrations of a single fine anterior filament, and is moderately quick, occasionally almost rapid. This may be long continued, but patient observation will show that, after some uncertain, wayward movements, they will gradually settle down, flatten out circularly, throw out pseudopoda, sometimes twice the diameter of the disk, and become in all respects perfect *Actinophrya sol* (Fig. 4). In this stage the true Actinophryan voracity is displayed. *Astasia margaritifera* abounded in

* *Vide* Mr. Waller's paper in 'Journal of Quekett Club,' vol. ii., p. 93; and President's communication to Reading Microscopical Society, reported in 'Mon. Micr. Jour.,' vol. iv., p. 334.

the water in which these incidents were first traced, one or more of which invariably fell victims to each Actinophryan, and in their metabolic efforts to escape, until they finally sank engulfed, presented a singularly interesting spectacle. This stationary condition is maintained for a longer or shorter time, the pseudopoda are then withdrawn, the former shapes resumed, and the onward vibratory movements renewed. I may add that they show a tendency to aggregate, and are mostly limited to a small portion of the field.

The question here arises, are we to regard these little organisms as independent forms, or as phases—and advanced ones—of Actinophryan life? I incline to the latter view; for may we not reasonably expect that, equally with creatures of a higher type of organization, the humble *Actinophrys* will fulfil those universal laws of nature which dictate that, no matter how frequent the propagation by self-division—no matter how numerous the parthenogenic descents—every living organism must sooner or later revert to the congress of two individuals, to secure the perpetuity of race by means of the fecundated germ, and that the fixed or feebly locomotive forms of animal life shall find in their actively motile embryonic developments the mode of effecting their dispersion in space.

VI.—*On the various Phenomena exhibited by the Podura Test under ordinary and extraordinary Microscopic Resolving Powers.* By G. W. Royston-Pigott, M.A., M.D., F.C.P.S., Cantab., late Fellow of St. Peter's College, M.R.C.P., F.R.A.S., M.R.I., F.R.M.S.

The variety and subtlety of this queen of test-objects under various treatment of illumination and magnified imagery strike the eye of every observer with astonishment, who examines the phenomena presented under different conditions of illumination, and different adjustments in the objectives and eye-pieces, and greater or less penetration or depth of focal vision. The microscopist with whom this precious, I would almost say, peerless object has been a prime favourite, possesses within it a complete *Thesaurus* of signs by which he can pronounce upon the performance of his glasses. In this very wealth of optical effects lies its chief value and interest. So long indeed as the object was made to show itself in one "standard" form, as the *ne plus ultra* of microscopy, and so long as both makers and purchasers of glasses conjointly decreed the standard test, and forbade, under penalty of contempt and ridicule, any other view, further advance appeared unnecessary.

The opticians were fairly credited with the most honest intentions when they constructed the glasses to show this standard and no other. They naturally desired to display microscopic power in its most approved and effective form as a *summum bonum* of art. The exclamation markings survived as a test for forty years. I am fortunate to possess an ancient scale of the Test Podura and one of Pritchard's doublets 1–80th of an inch focal length, capable of showing these veritable markings. Why the resolution of these markings into much more minute structure by the glasses of to-day, when these same markings could be seen forty years ago with a doublet, should be so much doubted, appears almost unaccountable; and offers an interesting phase in the science of microscopy. No such a thing happened when the lined objects, such as diatoms, gave way to squares and dots; more complete resolution was hailed with satisfaction as a sign of progress. Science is not without many examples of outcry against, and disbelief in, new truths.

I attach very much importance to the phenomena displayed by the Podura Test-scale for several reasons:—

1. They are a severe test for chromatic and spherical errors.

2. An objective and eye-piece which is competent to thoroughly resolve this test from its primary markings into its secondary and tertiary forms may be pronounced super-excellent.

3. The scale is eminently fitted also to display the amount of penetration of an objective, as it possesses structures which are placed in two different planes; the one nearer and the other farther from the objective.

4. The scale gives a new lesson in microscopy, teaching the observer how to deepen or regulate the planes of focal vision independently of mere focussing.

Indeed, objectives can be so adjusted and compensated as to enable the observer to view either the upper or the lower structure, or both structures at once.

5. The scale is further valuable as embracing almost every form of difficult definition: lines, points, beads or molecules, and brilliant disks of light, as well as the colours due to films of various thickness as shown in "Newton's Rings."

THE SHADOW PHENOMENA.

The standard (?) markings of forty years' pedigree may be seen with a good inch objective, or with a powerful doublet. A startling fact is here disclosed. *Only a very small angular aperture is requisite to display them.* The markings (! ! !) are therefore independent of large aperture. If we could imagine a Podura scale 1000 times larger than usual, there does not seem the smallest reason to doubt that we could see these markings with the eye alone at a

distance of ten inches, *i. e.* with an angular aperture of about *one degree.**

The club mark or note of admiration is therefore entirely different in its nature from a diatomic line-mark primarily made out by a third-rate objective. For these lines cannot possibly be defined with so small an aperture even as 15°: perhaps 30° is the limit. In proportion as the angle is enlarged, *cæteris paribus,* diatom markings come out more and more distinctly. This broad fact points to the conclusion that these Podura marks are *shadows;* they disappear with oblique light. Parallel rays perpendicular to the scale or coincident with the instrumental axis display them most clearly. A great variety of such shadows or obstructions of light may be shown.

(*a*) If a number of the *Formosum* diatoms be selected which lie across each other irregularly, a great number of black shadow-patterns are developed, easily seen with a low aperture and power, many of them very closely imitating the Podura marks or clubs. Of course where light cannot get through on account of intricate internal refractions and reflexions in a mass of brilliantly light-transmitting spherules, we get darkness or black markings.

(*b*) If we diligently search among slides rich in overlying *ribbed* scales, with a low power, we shall be rewarded with beautiful darkness-patterns in many forms; and the more nearly the ribbings of the superimposed scales are parallel to each other the longer appear the markings of the wavy group.

(*c*) Whenever minute cylinders of a light-refracting character lie athwart each other, black shadows are shown near the points of each intersection: with splendid definition these black shadows can be discerned even in the plume-hairs of the gnat at their points of intersection. Spun glass shows them in the highest perfection.

I regard, therefore, the black marks on the Podura seen with a low power and small angular aperture as simply jet-black shadows from the stoppage of light-transmission.

Some allusion must now be made to the phenomena approximately exhibited by minute transparent cylinders intersecting each other.

Sir David Brewster improvised fairly good lenses by placing two small cylindrical bottles filled with water at right angles to each other; their intersection formed a lens which produces a fair image of a distant object. In October, 1870, Mr. Moginie procured for me a number of fine glass rods; when these are placed in contact in one plane, and another similar set is also placed behind them at right angles, brilliant images of a distant flame are beautifully dis-

* In other words the base of a triangle, with the pupil of the eye for base and a point 10 inches distance for the apex, would subtend nearly one degree.

played, and as the angle at which the glass rods intersect is changed, distorted images are formed of great variety.*

Now when a scale is composed of *ribbing* approximately cylindrical, and when there is a double set, the one crossing the other, as in the Lepisma, an approximate definition shows a bead-like appearance at the points of intersection, as noticed long ago by Mr. Beck: these *quasi* beads are limited to the points of intersection, and are very sparse and few. A more profound and *recherché* definition dissipates these false beads and displays a surprising wealth of beading composing the entirety of each Lepisma ribbing in both sets (described by the writer in 1869).

Molecular Definition.—Whenever molecules, or spherules, or beads are arranged in straight linear rows and in close contact, whether in diatoms or scales (before the glasses receive those exquisite corrections necessary to disintegrate the cylindrical appearance into its component beading), their primary appearance under an approximate definition is always cylindricular, that is formed of *cylindricles.*

I may remark that some of the intersections on Lepisma near the edge closely simulate Podura markings; and the elongated beads described by Mr. R. Beck in this Journal, resolve themselves into straight clusters of minute beading; similar to those of the resolved *Podura clubs or markings.*

The molecular definition of these bodies is clearly dependent upon the definition of each molecule. The very delicate line of darkness formed by the points of contact between two adjacent molecules, and the tracery-shading produced by their curvature, offer some of the most severe trials of the delicacy of development possessed both by the eye and the instrument. Here we are landed as it were in a fairy shadow land. Too much light overwhelms the eye and obscures these delicate forms. The least practicable and a bluish light is the best. Sir W. Herschel recommended a quarter of an hour's absolute darkness before submitting the eye to view the faintest objects in the heavens: this rule is too much violated by the microscopic student. A dark room, a shaded eye, calmness, and above all, absence of prejudice, are essentials to microscopic research, not always attainable.

Mr. Slack's Silica Beads.—A new form of test is here given. On some of his slides isolated silica spherules are copiously visible. Singly observed, there is a bright halo round them with the best glasses; when two happen to touch, their distinct individuality is obscurely defined. If they were as small as Podura molecules or beads, and placed accurately in a straight row or rouleau, their reso-

* I left these with Mr. Curteis, who kindly fitted them up, and Mr. Hennah, of Brighton, subsequently photographed some interesting effects, which he did me the honour to present to me.

lution would be primarily cylindrical, secondarily molecular: as a third and higher order of excellence the bright band of the secondary spectrum would diminish in breadth, and each spherule would present under direct illumination a focal point of light.

Mr. Slack's Silica Films.—No more instructive object has been seen than these slides. First of all, we know what we are looking at and what we ought to see. When our Secretary, with the humour he so richly possesses, suddenly asked me at the Society's room what I saw on the microscopic stage, through an old Ross' $\frac{1}{4}$, I at once said, "Some cylindrical fibre, but what, I could not tell!" *He then informed me they were cracks in a film of silica*; yet they seemed to me absolutely cylindrical fibres. I have noticed the same appearance in Nobert's lines. Here we see what the microscope may misrepresent. The interval between the edges of the two separated films was filled up by a phantom-cylinder! Two square edges were thus transfigured. *It is evident that there ought to have appeared two black lines*, with more or less prismatic colour resulting from the usual refractions at right-angled prisms. This experiment is exactly the reverse of the former. There were no beadings, but cylinders conjured from square glass or silica edges!

When the definition of these edges was accomplished in the most perfect manner obtainable, two black edges with a secondary spectrum were distinctly observed with Powell and Lealand's best immersion $\frac{1}{8}$th and a variety of other glasses.

There is one other cause of colour observable. Let us suppose we have resolved the scale *Podura macrotoma* into ribs, and then the ribs into parallel rows of bluish and reddish beads: now rotate the scale slowly, and these colours disappear. Some are of opinion that these colours are due to diffraction, others to polarization of thin films. However that be, these double beads are generally invisible in perfection, except when showing nearly complementary colours.*

Molecular Ribbing and their Intersections.

Advancing now in the scale of difficulty and double intersecting rouleaus of beads crossing at various angles in contiguous planes, above and below, we get we will suppose:—

Primary Definition.—At the exact places where these rouleaus cross there is at first sight an obliteration. Where two spherules lie exactly the one over the other, a point of bright light is developed. In this way the Podura scale may be made to appear studded over

* Our late highly-esteemed President brought me this specimen to resolve for him; the charming delight which he displayed on seeing their complete resolution with a half-inch objective made a lasting impression of his unaffected amiability and candour. I subsequently had the honour of showing them to Dr. Lawson with an inch objective.

with bright points distant from each other about the length of eight or ten beads, according to the nature of the intersections.

With my Powell and Lealand's 1–16th immersion I develop alternate obliteration, and two short black lines intensely black, and between them, beading. In fortunate cases both our old friends, the clubs or *shillelaghs*, as well as two black lines and two different sets of beads besides, are visible.

I have never been able to see these double short black lines in any scales but Poduræ.

Subsidiary Forms presented by the Podura Test.

I. The lowest and worst form of development is the appearance of a few isolated beads at some of the intersections of the molecular ribbing. This can be readily produced by entirely deranging the corrections of a superb objective.

II. Another imperfect form of definition is a bulbous swelling, like a knot in a vein. A varicose swelling in surgery is an irregular bulging of the blood-vessel at irregular intervals. A few large and smaller bullets forced into an india-rubber tube give the idea of varicose swellings. But notwithstanding the frequent manner in which my distinguished friend, Colonel Woodward, has applied the term varicose as representing the appearance of the Podura ribbing, this description seems to me hardly to apply to an apparent ribbiness wholly composed of distinct beads closely packed together and separable into individual molecules. In the varicose idea there is continuity, in the beaded form there is juxtaposition, like spheres packed closely in an elastic and transparent tube, or like a necklace.

III. *A rippling undulation of beaded ribs*, as though a series of necklaces were placed parallel upon the small ripples often seen after the tide is out upon a sandy beach. This form appears by a change in the cover correction.

In observing Podura with the water lens and a cracked cover, the fluid is seen to insinuate itself in straight lines, precisely following the course of the beaded ribs; bead after bead disappears and reappears under a dry lens, as the fluid gradually dries up. In some cases I have observed pressure of the object-glass upon the slide (delicately administered) to cause the escape of an oily fluid which returns to the scale backwards and forwards as the pressure is changed by the fine focussing adjustment, and the course of this oil most clearly demonstrates the existence of *level* and not undulating beaded ribbing.

IV. The appearance of continuous ribs is the primary form of development. Colonel Woodward, with that acumen and perspicuity which so highly distinguish his communications, informs us that he has observed this valuable fact (long known to myself) that the

beaded ribs lie altogether above the exclamation marks or clubs. "The varicose ribbings are first seen, and the exclamation marks do not become distinct till after the scale is focussed through." The ribbing or streak is described by Mr. Beck in his work on the microscope, and declared by him also to be out of focus, a drawing of which is given (Plate VII., Fig. 4).* It was not then suspected that the true structure of the scale was put out of court, as it were, and that to see the "markings" distinctly we were obliged to focus absolutely through the structures, justifying the Colonel's words, "these exclamation marks are probably illusive or spurious appearances, notwithstanding the great distinctness they attain under favourable conditions." In confirmation of this opinion I may be permitted to state that these markings cannot be finely seen unless the objective is *spherically under-corrected.*

V. If one examines with a pocket-lens the several sets of photographs, so beautifully done by the accomplished Colonel, and which he has most handsomely placed at my disposal for reproduction, we find numerous instances in the Podura Test-scale of beautifully-formed spherical beads in close juxtaposition. It is difficult to imagine that different laws should govern the structure of the same scale. If it is distinctly beaded in one part, one can scarcely with propriety say it is merely varicose in another.

Postscript.

Now that the sun is low in a wintry horizon and easily available as a *direct illuminator,* I recommend, with great pleasure, to the inquiring majority of our Fellows the following *crucial experiment* on the Podura. Select a sunny aspect about 10.30 A.M. (with the sun about S.E.); arrange a Podura scale, the best you have, with its quill towards the S.E.; then incline your microscope gradually till the sun's uninterrupted rays impinge less and less obliquely upon the under surface of the slide. You will find a position, according to the angular aperture of your objective, when the scale becomes illuminated on *a dark field.* At that instant you will behold once for all, if your glass be of first-rate quality, the whole scale studded universally with a molecular structure hitherto unsuspected; but revealing at a glance the poverty of old-fashioned definition.†

* "The two appearances (Figs. 4 and 5) on one and the other side of the focus when the adjustment of the object-glass is incorrect." The very accurate drawing given unconsciously by Mr. Beck of the Podura ribbing is truly remarkable. The exclamation markings are seen in a different plane, and therefore out of focus. Figs. 4 and 5, in Mr. Beck's Plate VII., are well worth attention.

† As a first essay the young microscopist may be recommended to experiment at first upon the largely-beaded scales, as *Podura microtoma* and *Podura degeeria.*

PROGRESS OF MICROSCOPICAL SCIENCE.

Fungous Growths in Mollusk Shells.—In a paper read before the Manchester Philosophical Society on the 26th of February, Mr. Mark Stirrup exhibited sections of shells of mollusca, showing so-called fungoid growths. He referred to Dr. Carpenter's report on shell structure, presented to the meeting of the British Association in 1844, in which especial mention is made of a tubular structure in certain shells, and he cites the *Anomia* as a characteristic example. In the last edition of 'The Microscope,' Dr. Carpenter, he said, withdraws his former explanation of this structure, and now refers it to the parasitic action of a fungus. Mr. Stirrup showed sections of this shell penetrated by tubuli from the outer to the inner layers of the shell, and it is upon the inner layer that the curious appearance of sporangia, with slightly-branched filamentous processes proceeding from them, present themselves. The parasitic view is strengthened by the fact that these markings are not found in all parts of the shell, and are certainly accidental. Professor Kölliker maintains the fungoid nature of these tubuli in shells as well as in other hard tissues of animals, as fish scales, &c. Wedl, another investigator, considers the tubuli in *all* bivalves as produced by vegetable parasites, and that no other interpretation can be given. This view does not seem to be borne out by the section of another shell which was exhibited, "*Arca navicula*," in which the tubuli are always present forming an integrant part; they are disposed in a straight and tolerably regular manner between the ridges of the shell; moreover, they have neither the irregularly-branched structure nor the sporangia.

A Conspectus of the Diatomaceæ.—The 'Lens,' which is the quarterly journal containing the Transactions of the State Microscopical Society of Illinois, has at length, after long delays, caused of course by the terrific fire at Chicago, made its appearance. We must congratulate the Editor and Publishing Committee upon the fact. Of course it was difficult to get out a first number in a place so grievously injured by fire as Chicago; still it is a very good number, and we have no doubt that its successors will surpass it in excellence. The most important paper is one taken from the 'Monthly Microscopical Journal,' and it too is the only paper which possesses a plate, also taken from the 'Monthly Microscopical Journal.' Still there are some good essays, and among them is that whose title heads this paragraph. In this the author, Professor H. L. Smith, of Hobart College, N.Y., attempts to give a synopsis of the names of every genus of the Diatomaceæ, and the arrangement is apparently excellent. The explanation of the terms in use is a good one. It seems to us though that the author would have done better had he admitted fully the appearance of striæ as only an apparent structure that microscopic observation tended to show was merely a linear arrangement of dots or hemispheres.

The 'American Naturalist' on the dispute between Mr. Wenham and Mr. Bicknell, which was conducted in the pages of the 'Monthly Microscopical Journal,' is somewhat severe on Mr. Wenham. The writer, who signs himself C. S., and who is therefore easily recognized, says, that for some three or four years some American microscopists have been calling attention to the deception commonly practised by most working opticians in calling the "'power of their instrument less than it really is—*i.e.* calling an objective a quarter-inch when its focus is really but one-fifth or one-sixth of an inch, or an eighth when actually one-ninth or one-tenth,—and some now approach to one-twelfth.' In the 'Monthly Microscopical Journal' for December, 1871, Mr. F. H. Wenham writes a paper in reply to one of Mr. E. Bicknell's on this subject, in which he takes Mr. Bicknell to task for exposing the deception,—and admits the truth of the charge. Here we have a gentleman, well known throughout the microscopical world as one of the most accomplished *theoretic opticians* of London, generally supposed to be the principal adviser of the working opticians, not apologizing for, but practically defending the imposition, one that has been exposed and complained of by Dr. Wm. B. Carpenter.' Mr. Wenham says 'a scientific microscopist gives the diameters with his illustrations and the nominal power of his object-glass; this quite meets the case.' In this Mr. Wenham is entirely wrong; it does not meet the case. A power of one thousand diameters obtained with a one-inch objective is a very different thing from one thousand diameters obtained with a one-tenth, *unless the one-inch is ten times as good an instrument as the one-tenth.* The scientific microscopist should give with his illustrations, not only the amplification he employed, but the real focus of the objective, and the name of the maker, as astronomers do in the case of their telescopic observations. He further says, 'In such a difficult and complex arrangement as a high-power object-glass, it is almost impossible for all the makers to work to the same magnifying standard.' That of course depends on the knowledge of optics possessed by the workman, but has nothing to do with the matter. When the object-glass *is made*, the focus can be measured, and the glass named accordingly. The nearer the actual power comes to that intended, so much the more credit to the maker; the further it is from what he sells it for, the more to his discredit. It is an axiom in microscopy that the lower the power of a glass that will give a certain result or effect, the better the glass. Mr. Wenham's comparison with the steam-engine is as inappropriate as Hartnack's objection to English microscopes, that with their wheels and screws they look like a steam-engine."—*American Naturalist*, February.

The Muscular Fibres of the Small Bronchi.—On this subject Professor Rindfleisch, in the 'Centralblatt' of February 3rd, makes the following remarks, which are thus briefly abstracted by a contemporary:—1. The smallest bronchi have a special and distinct layer of transversely-disposed muscular fibres, which, in the places where the former debouch into infundibula, are specialized into a sphincter; they are capable of considerable dilatation, and have under their epithelial layer a finely-meshed network of capillaries,

like those of the lungs. 2. The circular fibrous tracts of the smaller bronchi send loop-like processes into the mouths of the infundibula, which penetrate to the fundus of the same. At from two to four points, tracts of smooth muscular fibres surround the infundibula in an annular form; these muscular rings lie mostly in the inwardly-directed portions of the alveolar septa. 3. All these muscles are in the so-called brown consolidation of the lung, in a condition of hyperplasia; those who have seen them in morbid specimens will be able to recognize them, though they are very delicate, in normal lungs.

Nemosoma elongata.—Mr. Joseph Sidebotham, F.R.A.S., read a paper on this species at a recent meeting of the Manchester Phil. Society. The author having discovered a considerable number of specimens of this very rare species under bark of elm, at Beeston, Notts, in November last, and having the opportunity, carefully observed its habits, of which he gave a detailed account, illustrated by specimens and by portions of bark and diagrams; showing also specimens and drawings of *Hylesinus vittatus*, on which it is parasitic.

The Diatom Hoax.—Our readers may remember our referring to an article some time ago, in which the most monstrous microscopic absurdities were broached. We certainly did not consider its matter to be at all amusing. However, the 'American Naturalist' of February considers that it was a hoax. Speaking of it, it says, many readers have enjoyed, in a late medical journal, the ingenious essay on test-objects, in which the new immersion one-seventieth of 191°, wet with fluoric acid and illuminated by a new eccentric parallelopiped with fluorescent rays exclusively, is represented as revealing that the structure of *Pleurosigma angulatum* is like the Nicholson pavement; and that a new diatom, fortunately rare, has beads more than one hundred and forty-seven millions to the inch, which are invisible by all other lenses and to all other observers. They will be further amused by learning from the 'Boston Journal of Chemistry' that some foreign medical journals have seriously reviewed this burlesque and discovered it to be a hoax.

Absorption of Insoluble Matter by Animal Membranes.—This subject, which cannot be worked out without the help of the microscope, has lately had a series of experiments made upon it by Dr. Auspitz, of Vienna, who has arrived at the following conclusions on the subject:—1. That, in mammals, insoluble matter (starch-flour granules), starting from the peritoneum and subcutaneous tissue, is able to reach the lungs, and through these organs to enter the general circulation. 2. That these granules, in order to go over into the veins, pass through the lymphatic system. (That they are taken up exclusively in this way, is not as yet proved.) 3. That the epidermis always presents a considerable, though only relative and not absolute, obstruction to the absorption from the integumentary surface. 4. That the absorption is essentially promoted by the mediation of fat, which goes over into the circulation in the same manner as starch-flour, though even more easily. Finally, the supposition may be offered, even if the direct

proof is provisionally deficient, that all that is true of starch-flour, and in a higher degree of fat, may also be asserted of other insoluble bodies of finer division and, therefore, less permanence of form than the starch-flour. The supposition is not in any way contradicted by the discoveries of Auspitz, made in connection with his well-known inunction experiments with mercury.

The Examination of Sponges under the Microscope.—In a paper published last year in the 'Annals of Natural History,' and which is remarkable, as it is the paper in which Mr. Carter, F.R.S., demonstrates the true form of the sponge-cell (on which we published Prof. James-Clark's paper in our last number), the author gives some hints which may be useful to our readers. He says that, for the most part, all marine sponges (save the *Clionidæ*, which may be in deciduous shells) begin to perish within forty-eight hours after they have been taken from their natural habitat, although their attachment to the piece of rock on which they may be growing remains uninjured; and even if they survive a little this period, they are voraciously devoured by the crustaceans which may be confined with them—just as in all similar and serial microscopical inquiries, whether free or confined, the minute crustaceans are thus the most defeating agents. With the putridity or dissolution of the sponge comes a development of infusoria; and if, under such circumstances, *one* Vibrio is seen to pass across the field, the microscopist may as well give up all further research into the phenomena of the *living* sponge. On the other hand, if the seed-like body be taken from a living piece of *Spongilla* and placed in a watch-glass with water, it may be kept under a quarter-of-an-inch compound power until the young *Spongilla* issuing from it has gone through all its phases of development from its first appearance to its full completion, which may be seen both elementarily and collectively; while during this time, having a plurality of seed-like bodies growing in different watch-glasses, the experiment of feeding the young *Spongilla* with carmine or indigo, which soon points out, by its colour, the position and grouping of the sponge-cells, together with the passage of the particles in through the pores of the dermal sarcode, thence to the ampullaceous sacs, and then the discharge of the ingesta through the excretory canal system—all may be deliberately watched under the same microscopic power, with so little difficulty and yet so accurately, that there is no merit whatever in recording observations of the whole process. It was in this way that Mr. Carter obtained the data published in his paper "On the Ultimate Structure of *Spongilla*," confirmed by similar observations on large pieces of *Spongilla* taken directly from the tank.

The Centripetal Fibres of the Spinal Chord have been recently submitted to some experiments by Dr. Dittmar. Rabbits under the influence of woorara were the animals experimented on, and the experiments which he carried out led him to believe that it is certain that there is a system of fibres in the substance of the spinal chord which, though they do not belong to the nerve roots, are capable of responding to the action of direct stimuli, and can transmit the impulses thus generated along the whole length of the spinal chord

up to the medulla oblongata, where they undergo reflexion into motor nerves.

The Vienna Syphilis Corpuscles.—A medical journal, quoting from the 'Allg. Wiener Med. Zeit.' of February 6th, says that the latter contains a serio-comic article on this discovery of Lostorfer, which, for a few moments, shook such men as Stricker, Hebra, and Skoda off their balance. Dr. Wedl, the author of the article, says:—"These corpuscles will, at the next meeting of the Medical Society, be solemnly buried in Dr. Stricker's museum. The sympathy of the profession is requested under these painful circumstances, and the Society will doubtless institute special masses for the repose of the departed. It is very lucky that the members did not, in their hurry, have medals struck for the discovery; and there is time left to send counter orders to Paris and London, to stop enthusiastic researches which might bring some blame on the Vienna Medical Society. The latter may derive from this mishap the lesson to beware of allowing itself to be made a trumpet to ephemeral discoveries." The 'Lancet,' however, finds it difficult to believe—and we quite coincide with it—that observers like Stricker and Hebra would have been carried away by imperfect experiments. It is clearly stated that different kinds of blood were placed under Dr. Lostorfer's microscope (he not knowing whence the blood came), and he constantly recognized his peculiar corpuscles in blood coming from patients affected with syphilis. Time will show who is right.

Alternation of Generations in Fungi, taking place in different Plants.—This phenomenon, though asserted to occur by Professor Oersted, is disbelieved rather by Mr. Cooke. He believes it takes place in the same plant as in the case of *Bunt*, but he feels great difficulty in believing in this process, where the generations were passed in different plants, until confirmed by other observers. If the spores of *Æcidium Berberidis* were taken from the Barberry and sown upon young wheat plants, and all these plants became infected with corn mildew (*Puccinia graminis*), to which wheat is but too prone, it certainly seemed premature to say that the spores of the *Æcidium* caused the *Puccinia* to be developed as a second generation; whereas it is much more probable that the germs of the mildew already lay dormant in the wheat, and, at most, the sowing and growing of the *Æcidium* spores only stimulated the mildew to a more rapid development. He said, at one of the meetings of the Quekett Club, that he certainly thought such a theory more probable, and quite as sound as the other.

Hydra carnea in Lake Superior, U.S.—The United States' Lake Survey Report has not yet been issued, but a portion of the zoological records have been published in 'Silliman's Journal.' From it (by S. I. Smith and A. E. Verrill) we learn that this beautiful *Hydra* (agreeing with Ayer's description of this species) was very abundant at the eastern end of St. Ignace, upon rocks along the shore and near the surface, frequently completely covering quite large surfaces where they were protected from the direct sunlight, and was

also brought up in many of the dredgings from 8 to 148 fathoms. In 32 fathoms, Neepigon Bay, and in 59 fathoms, off Simmon's Harbour, it was brought up in abundance from a soft clayey bottom. In the deep dredgings, it frequently came up near the bottom of the clay in the dredge, and was evidently not caught while the dredge was near the surface.

Transmutation of Form among Protozoa.—Professor A. M. Edwards, of America, has published some curious notes on this subject, which we shall probably reproduce in full in an early number. For instance, he records (as have some of our own microscopists also) the passage of an *Amœba* into a *Kolpoda* or *Paramecium.* Hence he considers the *Amœba* the young of *Paramecium.* This paper contains other matters of importance, which we must defer noticing at present.

Foraminifera of the English Chalk.—Dr. W. K. Parker, F.R.S., our President, and Prof. T. Rupert Jones, have published, in the 'Geological Magazine' for March, a paper on the Rev. Henry Eley's work on the above. The paper is little more than an emended notice of Mr. Eley's work, so that those who have not his work may profit by his views. It is well worth referring to by those who are interested in this particular branch.

NOTES AND MEMORANDA.

Microscopical Diagrams.—A large lot of these, 180 in number, size about 22×16 inches, are about to be sold in America; it may interest some of our readers to know of it. They are finely executed by a well-known English microscopist, and were prepared to illustrate a series of lectures on Fungi, Foraminifera, Polycistins, Seeds, Infusoria, &c., &c., &c. They are mostly painted on tinted paper in water colours, so as to give good contrast and be readily seen. Many of them represent unique specimens observed by the author, and are of the greatest value to the student of the lower forms of animal and vegetable life. Address, Naturalists' Agency, Salem, Mass., U.S. of America.

Changes of Current in Lakes affect Distribution of Diatomaceæ, &c.—The 'Lens,' the first number of the American Microscopical Journal which has been promised so long, contains an account of a paper by Mr. Babcock, "On Chicago Hydrant Water," showing that shortly after the course of Chicago river was reversed by the deepening of the Michigan and Illinois Canal, there was a marked diminution in the amount of organic matter in the hydrant water. He accounted for the change on the theory that the southerly lake current flowing along the western shore had brought to the crib the lighter vegetable products of the northern streams, and that the diversion of the river channel, by causing this current, opposite the city, to swerve slightly

to the west, had admitted to the crib the purer water from the deeper portion of the lake. He based this theory mainly upon the fact that while the hydrant water had never contained any of the stipitate diatoms growing in abundance along the shore, the free unattached forms were always present, together with several species whose origin had not previously been determined, but which he had found in abundance in several of the more northern streams.

Health of Professor Huxley.—Our readers will be glad to learn that Professor Bence Jones, the medical adviser of the Professor, asserts that the statement that the Professor has shown a tendency to phthisical disease is quite unfounded in fact. Mr. Huxley has never at any time exhibited any disposition to lung mischief. He is suffering from overwork only, and there is every reason to expect that he will soon return to England in good health. He gave up his place at the School Board in order to spare himself on his return.

How to make Sections of Small Fragments of Soft Tissues.—The following facts are stated by Dr. Woodward in the new number of the 'Lens'; most of them are, however, familiar to English workers. Where the fragment is too small to be conveniently held between the finger and thumb of the left hand while the knife is guided by the right, it may advantageously be imbedded in some suitable substance. In the German laboratories a mixture of wax and oil is extensively used for this purpose, and has been highly lauded. At the Army Medical Museum we have long since learned to give the preference to paraffine, as more readily handled, more cleanly, and yielding altogether better results with less trouble. A sufficient quantity of paraffine having been melted over a spirit-lamp in a small porcelain capsule, or any suitable vessel, is poured into a mould made by folding a piece of ordinary writing-paper into any convenient form, and the fragment of tissue is held in the middle of the fluid with a pair of forceps until the cooling paraffine is hard enough to retain it in place. It is then left to itself until the paraffine is quite cold, when the paper is unfolded, and sections can readily be made by cutting through the paraffine in any desired direction. The sections as made are dropped into alcohol, in which the tissue is readily disengaged from the paraffine by a pair of small forceps.

Collins's Light Corrector.—A very useful piece of apparatus was exhibited by Mr. Collins (17, Great Portland Street) at the Quekett soirée, consisting of a brass stage-plate with a groove in which rotates a diaphragm of four apertures, one of them being open and the other containing blue glasses of special tint, and one with a finely-ground surface. These effectually correct the yellowness of all artificial illumination, making the light soft and agreeable to the eyesight as well as improving the definition. It is in fact an improvement on Rainey's Light Modifier so as to obtain more varied effects, and does not require any special fitting, as it can be used on any microscope.

CORRESPONDENCE.

Mr. Cundel's anxiety for the Credit of this Journal and Sir David Brewster's Scientific Reputation.

To the Editor of the 'Monthly Microscopical Journal.'

Mr. Editor,

Sir,—Whilst admitting the intended courtesy of Mr. Cundel's letter upon some remarks which I had the honour of addressing on the 3rd January last to the President of the Royal Microscopical Society, in which he appears to call me to account "for the credit of the 'Microscopical Journal,' and in justice to the scientific character of the late Sir David Brewster;" I may be allowed to reply, first of all, that it may be safely assumed that the credit of this Journal is quite secure under the protection of its present Editor; and, secondly, that no puny efforts of mine could possibly injure the world-wide reputation of Sir David, of whose writings I am an admiring student.

Now, Sir, though Mr. Cundel complains thus of my statements (rather curtly reported), I hope he is not a photographer, lest Sir David's own words should further on meet his unqualified disapproval. If Mr. Cundel would have given himself time and opportunity for reading Sir David's 'Optics,' he would have saved me the trouble of encroaching on your valuable space.

Sir David excited the wrath of photographers in general by his unsparing criticisms bestowed upon wide-angled lenses, and I shall make a few extracts from his writings to illustrate this point, in deference to Mr. Cundel's wish. Amongst other things Sir David says:—

"As the pupil of the human eye is little more than two-tenths of an inch in diameter, we may regard the picture on the retina as a correct representation of external objects, in so far at least as its correctness depends upon the size of the lens which forms the picture . . ."

"Let us suppose that a lens *four* inches square is employed to produce upon a plane surface the image of any object, and that the size of the pupil of the eye is two-tenths of an inch; then, as there will be several hundred areas equal to that of the pupil in the lens, the image given by the lens will be a compound image consisting of several hundred perspective views of the object taken from several hundred different points of sight, each distant two-tenths of an inch from its neighbour, and all those on the margin of the lens distant three inches and eight-tenths from those opposite to them. *Such a jumble* of images cannot under any circumstances be a true representation of the object."

"Let us now apply these results to the photographical pictures of the human bust as taken in a camera. The human face and head consist superficially of various surfaces, some vertical, some horizontal, and many inclined at all angles to the axis of the lens by which they are represented on a plane surface. . . . .

"The right-hand marginal part of the lens may introduce into the portrait the left eye, or the left ear, or the left side of the nose: . . . . the lower part of the nose, the interior of the nostrils, the lower part of the upper lip, and the lower part of the chin will be introduced by the lower marginal parts of the lens; while the top of the head, the upper parts of the lip and the eyelids will be introduced by the upper marginal parts of the lens. . . . .

"*A monstrous* portrait *of the human bust is thus obtained by the photographer, the monstrosity increasing with the size of the lens.* The nature and character of the portrait will thus vary with the superficial form of the lens, which may be circular, oval, square, rectangular, triangular, or of any irregular form; and in this way remarkable modifications of photographic portraits may be produced merely by varying the shape of the (exposed surface of) the lens. . . . The hideousness of photographic portraits is universally admitted . . . . the true cause modified doubtless by others, is the size of the lens, even if the lens is optically perfect." (Page 70.)

"The photographer, therefore, who has a genuine interest in his art will receive these truths with gratitude; and by accelerating the photographic process with the aid of more sensitive materials, he will be able to make use of lenses of very small aperture, and thus place his art in a higher position than that which it has yet attained. The photographer, on the contrary, whose sordid interests bribe him to forswear even the truths of science, will continue to deform the youth and beauty that may in ignorance repair to his studio, adding scowls and wrinkles to the noble forms of manhood, and giving to fresh and vigorous age the aspects of departing or departed life."

The next part of Mr. Cundel's letter, Sir, refers to the value of small aperture in microscopic object-glasses.

I wish to say here that Sir David Brewster's opinion upon this point has been quite as decidedly expressed elsewhere. It is when an object is in relief, presenting appreciable depth as well as breadth, that distortion may so illusively occur in wide-angled objectives. Dr. Carpenter has well shown this in his observation of the effect of a wide-angled objective in binoculars causing *spherical foraminiferæ* to appear standing up *egg-shaped.* Sir David expressed himself with his usual eloquence and decision on this very point, and he applies the principles of truthful photography to illustrate these effects.

"It has been demonstrated," says he, "that all objects in relief are misrepresented by large lenses when their pictures are taken in the camera obscura. The human face divine is caricatured. Parts invisible are displayed, and parts visible are deformed. When Polyphemus admired Galatea she was not the beauty who fascinated Acis: and we think a national reward should be offered to the daring Ulysses who should extinguish the orb of every photographic Polyphemus in the land. But if the photographic lens thus deforms youth and beauty and age, and even trespasses on inanimate nature, what may we not expect from the Cyclopean eye of a twelfth-of-an-inch object-glass viewing microscopic objects in relief several thousand times less in diameter, and so near, it would see nearly the whole surface, were it a sphere? For the purpose of illustration, we may suppose the micro-

scopic object to be the head, in relief, of the Venus de Medicis, on a much smaller scale than the beautiful microscopic portraits of Mr. Dancer, of Manchester, and that the microscopical observer is requested to make a drawing of it. We cannot venture to say what would be its expression, but we are sure that it could have no resemblance to the original, both ears being fully brought out and almost the whole round of the head. In like manner every ridge in a microscopic object will show to the observer both its perpendicular sides as well as the side opposite the eye, and the resulting picture will be a coincident combination of a thousand different pictures, as seen from every point of the object-glass. The reason is therefore obvious why a large aperture shows lines that are invisible with a small aperture. The relief of a bust, or of a relievo either *basso* or *alto*, is best seen when we look at it in profile. Its height is then actually seen, whereas it is merely inferred when we look it full in the face. When the *raised* lines of test-objects are fully illuminated only obliquely, they are seen obliquely, and consequently much better than with a small aperture, which may not show them at all; not because the object-glass is inferior in *penetrating** power, but because the thing looked at in one case is not the thing looked at in the other, and is actually a smaller object."

Hence the perfection of a microscope consists in its having the "smallest angular aperture consistent with distinct vision. Such a microscope will not show certain objects of great minuteness, but it will give a perfect representation of what it does show. The large angular aperture will show the same objects and others more minute, but whatever it does show will be a mockery of the truth."

Mr. Cundel will find further remarks by Sir David Brewster on the necessity of small apertures for truthful portraits, in the earlier reports of the papers of the British Association.

I am, Sir, your obedient servant,

G. W. Royston-Pigott.

Does Cliona Burrow?

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—In your last number you were good enough to notice a paper of mine published in the 'Transactions of the Devonshire Association for the Advancement of Science, Literature, and Art,' 1871, "On the Boring of Mollusca, Annelids, and Sponges, into Wood, Rocks, and Shells."

At the conclusion of your remarks you say, "unfortunately for Mr. Parfitt," in allusion to a paper published by Mr. Waller in the 'Journal of the Quekett Microscopical Club.' Now, Sir, this exclamation of yours rather alarmed me; I thought I had either fallen into a tremendous error, or that Mr. Waller had discovered the whole

* This in modern language has been converted into resolving power, whilst penetration refers to depth of focus.—G. W. R. P.

problem of this mysterious boring question. I at once wrote off to a friend in London for the 'Journal of the Quekett Microscopical Club.' The return of post brought me the wished-for paper. I sat down at once and read it, and, Sir, what do you think my exclamation was when I had finished? why, like Charles Mathews in his celebrated character in "Used Up": he is supposed to have ascended Mount Vesuvius, and when asked what he thought of it, he simply said "*there was nothing in it*," and certainly there was nothing new in it. I cannot see that Mr. Waller has brought forward one single scrap of evidence to bear out his assertion that the Cliona does not make the excavations in which it dwells; he says that the holes are made by annelids: in some cases he is right, annelids do occasionally cross the path of a sponge, and the sponge takes advantage of the hole bored, or, as I have stated in my paper, "eroded."

When I was writing that part of my "Catalogue Fauna of Devon" containing the Spongiadæ, some hundreds of specimens passed through my hands, and each of these had to be critically examined. I consequently collected a mass of materials, and those bearing on the "boring" problem I used last year in the paper referred to. Mr. Waller says "we cannot possibly imagine a structure so feeble capable even of conveying the power of excavating at all without an entire subversion of the mechanical law, *viz. that an effect cannot be greater than its cause.* The feeble, simple character of this organism seems indeed to give us reason of its seeking for protection in holes and corners from external attacks."

We will place by the side of this two or three vegetable organisms, which I think will be admitted by all who know them that they are equally as feeble, if not more so, than this Clione in dissolving or burrowing into shell and rocks. The first, then, is *Verrucaria sublittoralis*, Leighton, which, curious enough, selects one or two species of acorn-shells for its habitation, the shells of which are sometimes very thickly perforated by it; and when the perithecia have fallen out of these holes, which they do when they get dry, and are sometimes washed out by the waves, and one seeing the shells then would declare they had been bored by some annelid. Pultney, and afterwards Montagu, both called one of these acorn-shells *Balanus punctatus*, thinking it peculiar to the species. The peritheciæ have the power of dissolving the shell and almost burying themselves in it.

Again, *Verrucaria calciseda*, D.C., has the power of sinking or burying its peritheciæ in the otherwise solid rock. And my friend Mr. H. J. Carter, in 'Ann. Nat. Hist.,' vol. v., 4th series, p. 79, says, in discussing Grayella, Osculina, and Cliona, "Undoubtedly, too, if the almost liquid mysogasters can work their way through hard wood to the surface; if the like delicate endophytes *Chytridium*, Pythium, &c., can pierce the horn-like coverings of Algæ, and the soft cell of *Zygnema* can dissolve its prison walls for exit and conjugation, the amœboid sponge can burrow amongst the layers of an oyster-shell for its subsistence." I think this is sufficient evidence to prove that equally *feeble* organisms have quite as great powers of working as is attributed to the industrious annelids, and the supposed indolent Clionas.

Mr. Waller endorses Dr. Bowerbank's view in reducing all the burrowing species of sponges enumerated by Mr. Hancock to the one *Cliona celata*. This statement is quite sufficient to convince me that Mr. W. has never seen another not uncommon species with similar habits to *C. celata*, namely, *Clione Northumbrica*, Hancock. This species is as distinct as any two species of a genus in all our British sponges, and I have no doubt but that Dr. Bowerbank will establish this as a species in his forthcoming volume on the additional species to his former volume on the British Spongiadæ.* There is, as I have stated in my paper, a very great similarity in the erosive workings of both annelids and sponges. The same may be said of the action of an acid on shell or limestone; and although I cannot prove by any test that these creatures use an acid for eroding their burrows, I feel convinced that such is the case. Take, for instance, the habit of *Halichondria suberia* and *H. ficus;* these species frequently invest the shell on which they attach themselves, and when this is the case, the shell becomes entirely dissolved, leaving its form entire in the inside of the sponge. The mere finding of an oyster-shell perforated full of minute holes, as stated by Mr. Waller, is not sufficient evidence, unsupported by facts, to convince anyone knowing anything of this subject.

Still pursuing this subject, I have added since I published my paper last year another fact in relation to the burrowing annelids.

In tracing the burrow of an annelid in an old shell of *Buccinum undatum* cast ashore at Exmouth, the burrow, I may say, was quite recent; it follows the direction of the whorls of the shell, keeping near the sutures; the hole is more or less irregularly excavated in a lateral direction, a transverse section of the burrow shows it to be nearly quadrate, with the above irregularity absent; but the most remarkable thing to me, and so far as I know it is new in the history of these burrowing annelids, is this, this burrow is regularly divided by transverse septæ. The diameter of the burrow is $\frac{1}{15}$th of an inch, and the septæ vary from the $\frac{1}{12}$th to the $\frac{1}{15}$th of an inch apart. The septæ are made up of very thickly-woven threads of what appear to be chitine. The object in building up these barriers across the burrow appears to me to be to protect the creature when at work, or to prevent the water diluting the erosive agent too much. Some water would naturally be left in the hole after the barrier was constructed, and this, no doubt, is quite sufficient for the creature's wants. But in this mode of working, the waste of material in building up these numbers of septæ must be immense on a small animal like this.

Unfortunately the annelid had escaped by a hole in the surface of the shell before I found it, I am therefore quite ignorant of the species which makes this remarkable burrow.

I am, Sir, yours obediently,

EDWARD PARFITT.

* See also remarks on this, 'Ann. Nat. Hist.,' 1870, pp. 80, 81.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *March* 6, 1872.

Wm. Kitchen Parker, Esq., F.R.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and the thanks of the meeting given to the respective donors.

A paper was read from Dr. Woodward, U. S. Army, "On Resolution of *Amphipleura pellucida* by a $\frac{1}{10}$ of R. and J. Beck, compared with other Objectives." The paper was illustrated by three photographs, taken by Dr. Woodward. The thanks of the meeting were accorded to Dr. Woodward.

In pursuance of the notice given at the preceding meeting, the ballot was taken for the election of Dr. George Charles Wallich as an Honorary Fellow of the Society, and the President announced his election.

Mr. J. W. Stephenson then read a description of "Stephenson's Erecting Binocular."

The thanks of the meeting were presented to Mr. Stephenson.

Dr. Klein then read a paper "On the First Stage of the Development of the Common Trout" (*Salmo fario*).

The President said he knew of nothing more interesting than the subject of Dr. Klein's paper. Anyone getting fairly started on the development of the embryo from the fecundative germ, had entered upon the most beautiful and interesting portion of science. The subject possessed charms for him which could be found in nothing else. In comparison with Dr. Klein's researches, his own seemed coarse and secondary. He wanted to know how the processes described took place; how the germinal mass through the amœboid particles could in some wonderful way fetch material from one part to carry it to another; what the fission of the primary parts meant. Numbers of such questions arose in the mind of the biologist in listening to such a paper as Dr. Klein's. His own work lay in the parts that formed right and left of the notochord, and in those parts that grew round the nervous centres; and he wanted it shown what was the difference between the parts that formed round the face downwards, and those growing downward from each side the notochord; what was the relation to the ear and the nasal sacs; and what the facial arches were; we did not know what to call them. A very interesting paper had come before the Linnean Society,† written by his friend St. George Mivart, on the laminæ that formed the *peritoneum*, as well as on those that formed the outer walls of the chest. All these were intensely absorbing researches, rivalling in wonder the questions that occupy even the astronomer himself. He (the President) had seen in Dr.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

† 'Trans. Lin. Soc.,' vol. xxvii., pp. 369–392, plate lxiii.

Klein's preparations the involution of the blastoderm which went to form the internal ear; and of that growth outwards of the nervous centre, the form of which might be imagined by picturing to one's self a dumb-bell, in which there was a central raised mass, besides the two terminal knobs, these outer lobes being the rudiments of the most important membrane of the eye. Then there was a horny layer coming over in front to finish it. There was thus a double process in the formation of the organ of vision. Many fascinating marvels would be witnessed in the formation of these special organs, in the ear not less than the eye; its first forms, then, the enlargement of the labyrinth and the closing of it in, and the construction of its different parts. It was a subject which the younger Fellows of the Society should take up and work out.

Dr. Klein then read another paper, "On the Nerves of the Cornea."

Mr. Stewart (in answer to a question from the President) referred to the observations made by him at a recent meeting, when the question was introduced whether nerves passed in the way described, and repeated his offer to show his preparation to anyone who wished to see them. In examining even the small trunks of nerves it could be seen that they were composed of a number of fine fibres, and these fibres could be traced leaving the main skein in which they were associated, somewhat in the same way in which it might be supposed a wire cable could be split up. These fine fibres would be seen to be really formed of a number of laminated fibrillæ, which left the main trunk of nerves in numbers of from 300 to 500, gradually unravelling so minutely, that no graver could represent them.

A vote of thanks was then unanimously accorded to Dr. Klein for the two papers he had read.

Dr. Klein then exhibited vertical sections through the blastoderm of the trout, 12, 13, 14, and 18 days old, and some very beautiful preparations of fine nerve fibres of the frog's cornea, and also of sub-epithelial and intra-epithelial fine nerve fibrillæ of the rabbit's cornea.

The meeting was then adjourned until the 3rd April next.

Donations to the Library and Cabinet from February 7th to March 6th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Journal of the Linnean Society, No. 66	*Ditto.*
Quarterly Journal of the Geological Society, No. 109	*Ditto.*
Berichte des Naturwissenschaftlich-medizinschen Vereines in Innsbruck	*University Buchhandlung, in Innsbruck.*
Om Siphonodentalium vitrium, &c., af Dr. Michael Sars	*L'Université Royale de Norvège, Cristiania.*
Mémoires pour servir à la connaissance des Crinoïdes vivants, par Michael Sars	*Ditto.*
Carcinologiske Bidrag til Norges Fauna, af G. O. Sars	*Ditto.*
Three Photographs of *Amphipleura pellucida*. By Dr. J. J. Woodward. U. S. Army	*Author.*
A Safety Stage for Ross' Microscope	*J. W. Stephenson, Esq.*
Four Slides of Mycetoma	*Jabez Hogg, Esq.*

The following gentlemen were elected Fellows of the Society:—

Phineas S. Abraham, B.Sc. and B.A.
A. de Sonza Guimaraens, Esq.
Robert King, Esq.
Henry E. Symons, L.R.C.P. Lond., M.R.C.P., &c., and
George Charles Wallich, M.D., an Hon. Fellow.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

January 25th.—Microscopical Meeting. Mr. Hollis, President, in the chair.

Mr. R. Glaisyer announced the receipt from Mr. R. Beck, of London, of a test-slide of Podura for the cabinet.

Dr. Hallifax, who introduced the subject for the evening, "Parasitic Plants," remarked that a distinction must be drawn between those which simply attached themselves to organized beings, and those which lived at the expense of the plants and animals on or in which they grew, for they were both external and internal. The ivy, which attached itself by its suckers to a wall equally with a tree, and did not live on the juices of the plant, was an epiphyte, whereas the mistletoe, the best example of a true parasite, obtained its food at the expense of the tree on which it grew, although the tree did not seem to suffer. The mistletoe seeds, surrounded by a viscous substance, were, it is believed, carried by birds, became attached to the branches or trunks of trees, and there germinated, sending their fibres through the bark into the sap-wood, and they, continuing to grow at the same time as the tree, penetrated deep into its substance, and received from the plant its juices. With the exception of its not possessing true roots, it differed in no respect from any other perfect plant, for it elaborated the crude juices obtained from its host into woody fibre, &c., of a different character from that of the tree in which it grew. Thin sections, cut through both, showed that the cells lay side by side, different in character and not coalescing.

Of a more minute character were some of the fungi, for though many grew on organisms they did not make their appearance until decay and death had set in; but some, like the potato fungus, not only grew on the living plant, but completely destroyed it, penetrating into all parts of its substance. Other parasitic plants were the orobanches; but lichens simply attached themselves to plants for support.

Mr. Wonfor remarked that to the pseudo-parasites might be added the bird's-nest orchis (*Listera nidus-avis*), which did not appear to grow upon the roots of the beech, but only under beech-trees. He had never been able to trace any attachment to the roots. The dodders appeared to differ from some of the other parasites, for their seeds germinated in the soil and then attached themselves to the plant on which they grew, not only sending roots into the substance, but twisting and twining round it like the ivy. Hence they had been called stranglers.

Of fungi which grow on and at the expense of animals, he might

mention those which attacked living flies or moths and glued them to a window-pane, the caterpillar-fungus of Australia, the fungus-foot of India, ringworm, &c.

As regarded the mistletoe, there was no doubt birds were the chief agents in its propagation, for it was generally found high up on trees. One very curious fact must have been noticed, *viz.* that its most active period of growth was during the winter months, when the trees were said to be dormant. Then it was it flowered and ripened its fruit.

There had been great difference of opinion whether the Druids held sacred our *Viscum album*, or the *Loranthus Europæus*, commonly found in oaks in the south of Europe, whereas the former was now but rarely found on the oak; in fact, some, in ignorance of its existence, went so far as to say it did not grow on the oak in England; but examples could be found in all oak-growing districts. People seemed to forget that it was not the mistletoe, but the oak-growing one which was held sacred, and which, from its comparative rarity, was searched for with great care. The greater proportion of the mistletoe sold at Christmas came from the Channel Islands and France, where its berries were produced much earlier than in England. While it grew on a great number of trees, the apple-crab, poplar, and ash were the most common.

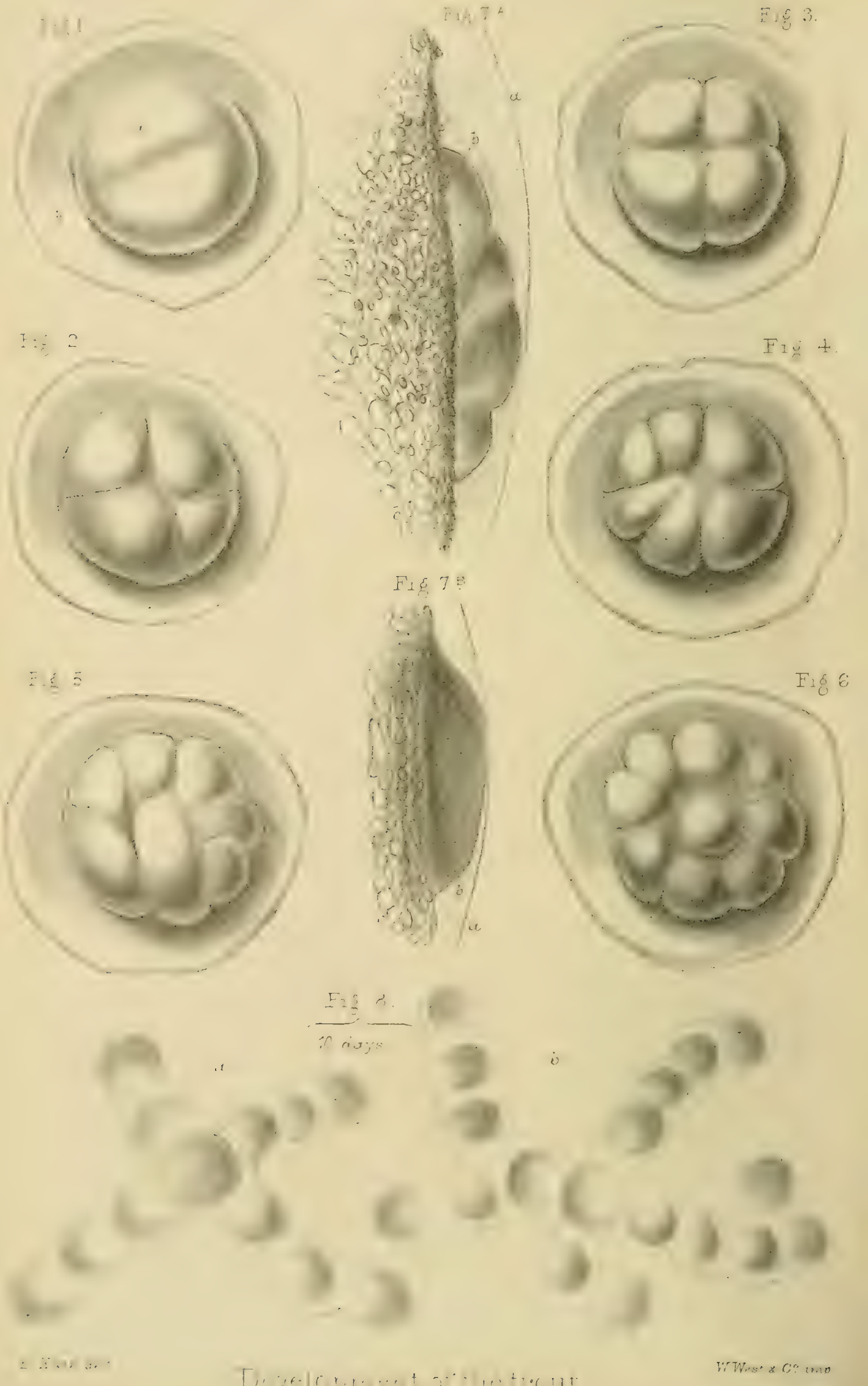
Mr. G. Scott mentioned that he had seen it on the laburnum.

Mr. Sewell had seen it growing on fuchsia trees in Jersey. An example of an internal parasite was the *Sarcina ventriculi.*

The meeting then became a conversazione, when Dr. Hallifax, Messrs. R. Glaisyer, Sewell, and Wonfor, exhibited male flowers of mistletoe, containing pollen, sections of the seed, leaf, stem, and of mistletoe and crab growing side by side, the caterpillar fungus, the potato fungus obtained in the autumn of 1865, and other interesting examples of parasitic plants.

Mr. Wonfor exhibited a very ingenious slide for opaque objects, consisting of a thin wooden slide, with circular cell turned and blackened, and having two very shallow grooves around, one for the covering glass, and another for a circular patch of gummed paper to fasten the cover. He had obtained them of Mr. Baker, Holborn, who supplied a dozen slides, with covers and patches to match, at 1*s.* 6*d.* per dozen.

In the account of the soirée of the Brighton and Sussex Natural History Society, in our last number, the names of Messrs. R. and J. Beck, as exhibiting some very exquisite photographs of microscopic objects, and of Mr. Hennah, as exhibiting a microscope and objects, were accidentally omitted. The omission was altogether due to the gentleman who furnished the report to the Editor of this Journal.



Development of the trout

THE

MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1872.

I.—*Researches on the First Stages of the Development of the Common Trout (Salmo fario).* By Dr. E. Klein.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

Plates XVI. and XVII.

Rusconi* was the first to describe the segmentation of the blastoderm of the egg of Teleostean fishes. His observations were made on the Cyprinoids, and he states that the mode of segmentation is quite of the same type as that of the Batrachia, known by Prevost and Dumas in 1824, and by Rusconi himself in 1826. The statement of Rusconi was confirmed by Lereboullet† for Pike and Perch;

EXPLANATION OF PLATES XVI. AND XVII.

Development of the Trout.

Figs. 1, 2, 3, 4, 5, and 6.—Showing the different stages of segmentation of the blastoderm. *a*, blastoderm. *b*, yolk.

,, 7 A and 7 B.—Showing the blastoderm of different stages in the profile. *a*, vitelline membrane. *b*, blastoderm. *c*, fat-globules of the yolk.

Fig. 8 A, 1 and 2.—Elements of the blastoderm of four days.

,, 8 A, 3, 4, and 5.—Elements of the blastoderm of seven days.

,, 8 *a* and *b*.—Elements of the blastoderm of ten days.

,, 9.—Vertical section through the blastoderm of three days. *a*, blastoderm. *b*, yolk of the saucer-like depression.

,, 10.—Vertical section through the blastoderm of six days. *a*, blastoderm. *b*, yolk. *c*, fat-globules of the same.

,, 11.—Vertical section through the blastoderm of twelve days. *a*, blastoderm. *b*, segmentation cavity. *c*, large elements on the bottom of the cavity. *d*, superficial layer of flattened cells of the blastoderm. *e*, yolk.

,, 12.—Vertical section through a blastoderm of fourteen days. *a*, segmentation cavity. *b*, peripheral swelling of the blastoderm. *c*, superficial layer of flattened cells of the blastoderm. *d*, lower layer of cells of the thinner part of the blastoderm. *e*, large elements on the bottom of the cavity. *f*, yolk. *g*, fat-globules of the yolk.

,, 13.—Vertical section through the embryo of eighteen days. *a*, superficial or corneous layer. *b*, second or nervous layer. *c*, third or motoric-germinative layer (middle layer). *d*, lower or epithelio-glandular layer. *e*, yolk. *f*, central nervous system. *g*, chorda dorsalis.

* Müller's 'Archiv,' 1836.

† 'Annales des Sciences,' 4 S., T. 1, 2.

it has been further confirmed by Coste * for Gasterosteus and by Carl Vogt for *Coregonus palea*. Stricker † has made assertions respecting *Salmo fario*, which do not agree with those of Rusconi.

According to Stricker knobs grow from the blastoderm in an irregular series, which become separated by a constriction from the main body of the blastoderm; from the primary knobs grow up secondary ones, which likewise become separated.

According to Stricker the segmentation, that is the splitting of the whole blastoderm, is completed in forty-eight hours. Stricker came to these conclusions by studying eggs after hardening them in chromic acid.

I have studied the segmentation of the blastoderm of four sets of eggs of *Salmo fario* in the living condition, and I have arrived at conclusions, which are in many respects different from those of Stricker, and which agree as regards the first stages of segmentation with the observations made on other Teleostean fishes.

A fertilized trout's egg exhibits under the microscope, up to the sixth hour, a rather uniform aspect. The egg appears as a dull globe enveloped in a thick vitelline membrane, and shows irregularly-distributed fat-like globules.

The earliest time, at which I was able to perceive an indication of the blastoderm, was five hours and forty-five minutes after fecundation, when there appeared in one place a small, somewhat more opaque, irregularly-outlined spot, in the circumference of which the fat-globules had accumulated. If the breeding apparatus is kept in a cold place, as is generally done, about the tenth or twelfth hour after fecundation, the blastoderm seen from above, or better in profile, appears as a distinct mound, which lies on a saucer-like depression of the yolk: a space is consequently left between the vitelline membrane and the yolk (compare Fig. 7 A). In that part of the yolk which represents the bottom of the saucer-like depression lie accumulated the fat-globules.

As soon as the blastoderm appears as a distinct mound, there are to be seen on it very remarkable changes. If we observe a blastoderm carefully for a considerable time with a No. 4 Hartnack, we find at first that its border does not remain in the same condition, but exhibits in different places slight changes. On one place, for instance, where the blastoderm has been sharply outlined, its margin becomes more transparent and slightly projecting, as if the mass of the blastoderm at this point had sent out a flattened prolongation; after a short time the prolongation is retracted, and the general outline of the blastoderm becomes sharp as before. The same appearance occurs in different places. The prolongations of the blastoderm which appear and disappear are of very different

* 'Histoire de Dévelopm. d. Corps Org.'

† 'Sitzungsber. der Wiener K. Akademie der Wissensch.,' Bd. 51.

sizes. It is evident that these appearances are due to an amœboid movement of the blastoderm.*

Soon after the blastoderm exhibits the appearances above mentioned, there is seen on its surface a median clear stripe, which by-and-by becomes clearer; this clear stripe represents the first furrow of segmentation, by which the blastoderm becomes divided into two halves.

The blastoderm now seen from the surface appears as if notched at two opposite points.

Of the presence of the first furrow we may convince ourselves by looking upon the blastoderm in profile. If an egg on which the first furrow has been made out in the surface view is looked at in profile, which may be done successfully after some manipulation, we may perceive that the blastoderm exhibits only in certain positions a notch, by means of which it appears to consist of two knobs of equal size; if we turn the egg round, we observe that the more we approach an angle of 90°, as regards its previous position, the more the notch displaces itself, so that the knobs become of unequal size. If the angle of 90° is complete, the surface of the blastoderm appears quite smooth; that is, the blastoderm is now so situated that the furrow lies horizontally.

If we turn the egg again through an angle of 90° the blastoderm appears once more as if consisting of two knobs, that is, the blastoderm has such a position that the furrow stands vertically.

A few hours after the first furrow has appeared, there appears a second furrow vertical to the former: the blastoderm seen from the surface now exhibits a cross of clear lines, apparently such that the two quadrants of the one half are smaller than the two quadrants of the other (see Fig. 3). The blastoderm seen in profile now exhibits in all positions a notch; that is, it appears to consist of two knobs.

* Oellacher, in a recently-published paper about the germinal vesicle (Max Schultze's 'Archiv,' 8 Bd. 1 Heft), says on p. 14, "That the germ (of trout) raises itself from the yolk-groove shortly before the fecundation, contracts itself, and spreads out again: that has Stricker described ('Wiener Sitzungsber. der K. Akademie,' Bd. 51) and has attributed it to active contraction. . . . . I can only confirm these observations most decidedly."

To these very decided assertions I have to add the following:—

(1) Oellacher makes Stricker to say something that Stricker never has said. Stricker in the above-mentioned memoir concludes, from the peculiar appearances which chromic acid preparations exhibit and which he (Stricker) mistook as appearances of segmentation, that the blastoderm possesses the power of active movement, by means of which knobs grow up, which afterwards separate from the main body. According to Stricker this proceeding forms a material part of the segmentation. But that Stricker has said "that the blastoderm shortly BEFORE THE FECUNDATION raises from the yolk-groove, &c., &c.," of that I cannot find anything in the mentioned memoir.

(2) I allow myself to doubt at once whether it is possible to OBSERVE contractions of the blastoderm on an unfertilized ovum of trout; because, as I have already stated, there is nothing *to be seen* of the blastoderm in *the living condition* at this time, even not in the first hours after the fecundation.

There is to be noticed in some eggs a peculiarity of the first two furrows, which consists in this, that they do not cross each other at right angles, but form a double V, the points of which do not touch each other, but are joined by a straight line (see Fig. 2).

At the end of the first day the blastoderm of nearly all eggs exhibits the first two furrows; whether the blastoderm gets a third equatorial furrow I cannot distinctly say; I take it as very probable, because I have found that the blastoderm persists in the stage of the first two furrows a comparatively long time. The next stage I have noticed appeared thirty-six hours after fecundation, and consisted in this, that the blastoderm seen from the surface showed two quadrants in division, that is, six knobs (see Fig. 4).

Until the end of the second day we find the eggs in the different stages between this just mentioned and a stage in which the blastoderm showed, when seen from the surface, four whole furrows, that is, eight knobs. This was also the last stage which could be regarded distinctly as a regular one.

The blastoderm which I have looked at during the third day, in the surface as well as profile view, reminds me very much of what Stricker has described and illustrated, for we see that the knobs of which the blastoderm consists do not exhibit any regularity either in their position or in their size. I could not make out any regularity even when the blastoderm exhibited in the surface view only sixteen or twenty knobs.

Until the end of the fourth day, even when the blastoderm appeared on its surface like a mulberry, I could distinguish very easily the single knobs with a magnifying power of only 90 diameters, so that we can say that the segmentation is far from being finished.

However unequal the time may be in which the eggs of the same set become developed, I must still consider, from what I have seen in fresh preparations and sections, that the shortest period for the segmentation is nine days. Of course the segmentation can be accelerated, if we allow the eggs in very small numbers to develop in the ordinary temperature of a room.

The elements of the blastoderm between the fourth and the tenth day, which can be examined in the fresh state only by puncturing the egg and enclosing the blastoderm with oil after mounting it in the yolk itself, appear as pale finely-granulated bodies; in many of them there is a central vesicular nucleus-like body to be seen. The elements exhibit distinct amœboid movements: from the protoplasm of the body there grow out slowly hyaline knobs of different sizes, which after a short time are again retracted.

The elements of one and the same blastoderm exhibit a great difference in size. There are always a great many elements to be found apparently undergoing division (see Fig. 8 A).

The elements of a blastoderm of a later period (for instance ten days) appear somewhat peculiar in this respect.

If the observation is continued, we can see that some elements which have been at first of a spherical shape, become of a cylindrical shape; further, we can see that on the latter there appear transverse grooves at nearly equal distances, as if it would divide itself into a certain number of smaller spherical elements (see Fig. 8).

About the spreading out of the blastoderm on the saucer-like depression of the yolk; about the large elements which are to be found on the bottom of the segmentation cavity; about the growth of the blastoderm round the yolk globe like a cap, the peripheral part of which is thickened; about the foundation of the embryo at the peripheral thickening, I have only few things to add to what has been shown by Stricker.

Between the tenth and twelfth day the blastoderm, which in the last days of the segmentation has become much larger and thicker, is seen to grow very rapidly in breadth. A fresh egg shows the blastoderm in this stage more transparent at the centre than at the periphery. On a section we recognize that the central portion of the blastoderm is much thinner than the peripheral portion; the central portion does not touch the yolk, but is separated from the saucer-like depression of the yolk by a cleft. The elements, of which the blastoderm consists, are in the deeper layers of the central part as well as the peripheral part larger than in the superficial ones. The most superficial layer consists of very much flattened cells; on sections it appears, as if the blastoderm were covered by a thin layer of spindle-shaped cells. We recognize at the same time that the blastoderm is not raised from the saucer-like depression of the yolk for the whole length of the cleft, that is, the segmentation cavity, but rests on the latter from place to place, by means of columns of cells, as if by pillars. The cells of these columns—subgerminal processes—are larger than those of the blastoderm, and are coarsely granular: we find cells similar to these on the bottom of the segmentation cavity too; amongst these we recognize some, which are very much larger, and possess two, three, or more nuclei. There is nothing easier than to convince ourselves, with Rinek,* first, that these elements which lie on the bottom of the cavity are products of the blastoderm itself, that is, that just as the blastoderm is lifted from the yolk, there remain some elements on its saucer-like depression; secondly,

* Max Schultze's 'Archiv,' Bd. V.

that the elements of the subgerminal processes separate themselves from the blastoderm, and remain on the bottom of the cavity the more the blastoderm is lifted off; and thirdly, that the further the egg is developed, the more these elements lying on the bottom of the cavity come outwards, so as to increase the peripheral thickening of the blastoderm (see Figs. 11 and 12).

The more the blastoderm grows round the yolk globe, the more the former becomes thinner in its central part. If we look at a section through the blastoderm of fourteen days, we observe that the central portion of the blastoderm is composed only of two layers of cells; the superficial layer consists of flattened cells, the deeper layer of spherical cells arranged more or less loosely in a row. The former layer is continued into the similar one of the peripheral thickening, the latter into the second stratum of the peripheral thickening; here this second stratum contains one or two layers of more or less cylindrical cells. Below these two strata the peripheral thickening contains a few more layers, at least two of spherical, distinctly granular, larger cells, with one or two nuclei. The cells of these latter layers are continuous with the large elements which lie on the bottom of the cavity (in this stage of development of course only at the peripheral part of the cavity; in the central part of the bottom of the cavity there are no more or very rarely such elements to be found).

About the fifteenth or sixteenth day the blastoderm has grown round somewhat more than a quarter of the yolk. In fresh eggs as well as in those hardened in chromic acid the contrast between the thickened peripheral part and the rest of the blastoderm is very striking. In eggs hardened in chromic acid, and deprived of their vitelline membrane, the former appears as a whitish ring; the rest of the blastoderm is so thin that the fat-globules of the yolk shine through it. About this time we observe that at one spot of the peripheral thickening a knob-shaped prominence shows itself which slopes towards the centre. This prominence represents the first trace of the embryo. In the subsequent stages (one day after) the embryonal prominence has already increased so far that it stretches like a cord from the elevated border towards the centre of the germinal area. We may now also observe with a lens that the end directed towards the centre is rounded, and further that the embryonal prominence projects beyond the extreme contour of the elevated border of the blastoderm in the form of a small knob. At the point of junction of the cephalic third (*i.e.* the portion turned towards the centre of the blastoderm) with the rest of the embryonal prominence we observe a shallow groove, the first indication of a dorsal groove, which is continued towards the centre (the head) and towards the periphery (the tail) a short distance, becoming *more* shallow, so that it may be

Fig. 8 A1

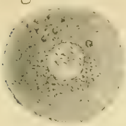


Fig 9.

8. A 3

8 A 2

8 A 4

8 A 5

Fig 10

Fig 11.

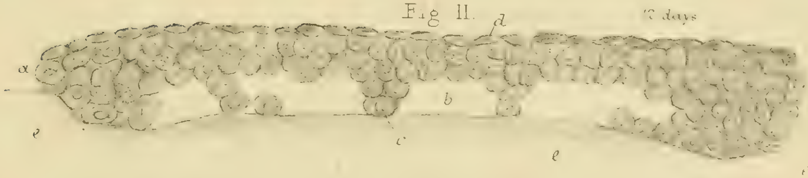


Fig. 12.

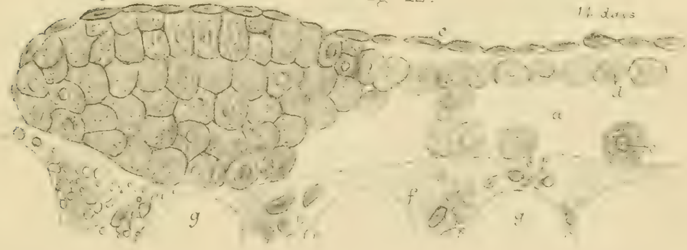


Fig 13

E. Klein delt

W. West & Co imp.

called rhombic. We observe further that where the groove is deepest a cord stretches from the embryonal prominence on both sides towards the centre of the blastoderm, where the cords of opposite sides meet, forming a ring which completely surrounds the cephalic extremity. This ring, which I shall name the secondary elevation, is somewhat thinner than the peripheral elevation, and at the time when I first saw it, it was so large, that its periphery, turned from the embryonal prominence, nearly reached the centre of the blastoderm.

In the immediately subsequent stages, until the period when the blastoderm has further grown round half the yolk, during which time the embryonal prominence has apparently become elongated, the circles increase in size, both the circle enclosed by the secondary elevation as well as that enclosed by the peripheral elevation; but both these elevations decrease in thickness.

From the stage when the peripheral elevation corresponds to the greatest circumference of the egg, the circle enclosed by the secondary elevation increases, whilst of course that of the peripheral elevation decreases, because the blastoderm now begins to grow round the other half of the yolk.

About the twentieth or twenty-first day the blastoderm has grown round the yolk so far, that a portion only of the yolk, no larger than the head of a pin, is exposed; the embryonal prominence is very long and thin.

Of a dorsal groove nothing more is to be seen. The peripheral elevation, now confined to a small ring, is just to be seen as such; the secondary elevation also, which now forms the equator of the egg, is just perceptible. Soon after (half a day to one day), the last indication of the peripheral elevation, as well as of the secondary elevation, disappears, and the last remains of the opening lying behind the tail of the embryo, on which the yolk was exposed, have disappeared. The blastoderm has become closed behind the tail, and has therefore grown *completely* round the *yolk*. We cannot therefore speak of the existence of an opening which should become the anus (*Rusconi* and *Stricker*).

If we make a thin section through the embryonal prominence of an egg seventeen or eighteen days old, which embryonal prominence has been hardened in very dilute chromic acid, we find that the embryonal prominence consists of the following layers: an upper layer corresponding to the corneous layer (Hornblatt); and then under this a nervous layer (Nervenblatt), consisting of several layers. Under this there follow two layers more (motorisch-germinatives Blatt and Darmdrüsenblatt), each of which consists again of two layers of larger cells. The nervous layer raises the

corneous layer in the form of two not very prominent elevations, so that a slight depression or groove results. We observe further that from the nervous layer a process extends deeply, and touches the chorda dorsalis, which has differentiated itself of the third layer, or motoric-germinative layer.

It is clear without more explanation that this median process of the nervous layer represents the central nervous system. (Compare Fig. 16.)

If we return to an earlier stage, or if we compare the sections which are made from nearer the caudal extremity, then we are convinced that the earliest indication of the central nervous system is always to be recognized as a solid process of the nervous layer, which is directed deeply, which process pushes before it the layer (lying under it in the middle line) more towards the yolk, and also becomes elevated towards the surface in the form of two elevations, which are, however, but slight.

Whether these elevations grow against each other so as to transform the slight groove between them to a canal, that is to say, whether the mode of establishing the central canal of the central nervous system is the same as in Batrachiæ and cheek, or whether the central canal, which is to be found in somewhat later stages in the central nervous system, develops as a split in the solid nervous process, I am not able to say with certainty; although a great number of sections from different periods were more favourable to the latter view than to the former.

II.—*On the Classification and Arrangement of Microscopic Objects.* By Dr. James Murie, F.L.S., F.G.S., &c.; Lecturer on Comp. Anat. Middlesex Hosp.; formerly Pathol. Glasg. Roy. Infirmary; Assist. Conservator Roy. Coll. of Surg., Eng.; and late Prosector to the Zoological Soc., Lond.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 7, 1872.)

*Part II.**

The present communication is but a continuation of a paper laid before this Society, and published in their Journal February, 1869. At that time it was my intention to have comprised in one article all that I had to say on the subject, and with this intent I drew up data which I subdivided accordingly. As it turned out, the matter was deemed rather voluminous for single insertion in the Monthly Journal of our Transactions, and what I may term Part II. was deferred. Meantime circumstances altered the tenor of my way, and the MS. then written out was put aside, only now to be revised. Excepting therefore this unavoidable delay, no material change in my original plan has been made, further than a revision of nomenclature in the main divisions of the vegetable kingdom. This is due to my friend Mr. Carruthers, of the British Museum, whose advice I have followed in the choice of that classification most accordant with the present tendencies of botanists, and freest from ambiguity.

In Part I. I divided my contents into nine sections, but of these only three were treated of. At this stage I merely repeat the three titles, as under-mentioned; and without further preliminary proceed therewith.

1. *Introductory.*
2. *Various Arrangements of Objects.*
3. *Microscopic Collection, College of Surgeons.*

4. *Principles of Classification.*—In Part I. I dwelt at some length on the modes of arrangement adopted by practical histologists or in use in existing collections, and on the breadth of purpose in amassing so much microscopic material.

Let us for a moment consider if there are any general rules or principles which it is useful to bear in mind; whatever be the classification chosen. Among others, the following appear to me worthy of consideration:—

I. It is fundamentally important not to multiply similar specimens needlessly; but rather by judiciously displaying them wheresoever most weight would be attached to their presence, concentrate attention on their real import.

* Part I. will be found in 'M. M. J.,' vol. i., p. 69.

II. It is of consequence that objects allied to each other should not, on feeble grounds, be placed far apart.

III. Keep as much as possible to a uniform style of nomenclature and size of slide, whenever the specimens admit of such.

IV. Endeavour to obtain good typical specimens, and see that they are in a sound state of preservation before finally giving them a position in the cabinet.

V. Reject all lumber, kept with the idea that numbers add to the value of a collection; whereas they rather weaken its usefulness.

These broad rules of guidance or general principles being admitted, it seems that the ultimate end of system in dealing with natural objects—and this is especially applicable to microscopic textures—is the attainment of a comprehensive grasp of the affinities and structural relations of things.

Around us everywhere matter exists in an infinity of forms, organic and inorganic. Materials building up organs adapted to perform functions ministering to the life and well-being of animals, or perchance supporting and maintaining vegetable organism. Materials wherewith minerals and precious gems are constructed, or elementary constituents out of which the chemist transforms substances applicable to the arts and sciences. In other words, the raw material of all useful and ornamental products, such as food, clothing, and microscopic art; also earthy substances of whatsoever kind, and intimate structures and organs of all vegetables and animals, recent and fossil. Indeed, everything likely to be examined by the microscope or retained as a preparation or interesting object in a histological collection, reduces itself within limits, when the elementary composition is unravelled. This limit is its primary textural component parts; and therefore this forms one kind of basis of classification to start from.

As, however, the simple elementary parts reconstruct themselves in a variety of ways into tissues and organs, and these tissues and organs again develop themselves so as to constitute systems; and as the systems are exponent in a great measure of the types of construction of vegetables and animals, we have in these so-called systems, normal and abnormal, a gauge wherein to measure and compare structural differences of two of the kingdoms of nature.

The third or inorganic kingdom comports itself more as to chemical composition, or the manner in which the masses are aggregated; so that instead of systems of organs, we have groups of substances to deal with.

Lastly, microscopic optical apparatus, art-microcosms, &c., are associated according to use, or whenever the subject agrees in bearing.

I mentioned that naturally the primary elementary tissues lead to a basic series, whereon a classification might be taken to start

from. These then would either precede or form the main source of divisions. On such grounds, most collectors of microscopic objects and writers on Histology form or dwell on many subdivisions of minute textural anatomy. Particularly was this the case with the late Professor Quekett, who dealt largely and classified extensively, according to the gradation of the elementary tissues.

Time-honoured and sanctioned on good authority as is the case, I nevertheless disagree with the custom of an elementary series preceding the more regular natural arrangement of objects in cabinets, except under certain conditions. I grant that introductory chapters on histological elements in manuals and text-books are not only useful, but a necessary prelude to the thorough understanding of the composition of organs forming bodies treated on. The circumstances of the case, however, are somewhat different, namely, between a treatise on histology generally and a cabinet of preserved specimens adapted for consultation and reference, where it is supposed the primary elements are to some extent mastered.

My reasons for divergence of opinion in the above point are:—

1. That a separate elementary series causes an unnecessary duplication of specimens; the same object being found in several places in the collection.

2. It mars the general harmony and sequence of the grouping.

3. The elementary tissues are in reality part and parcel of the main subdivision of organs and systems, and hence ought to be looked for therein.

4. The exception would be when a collection was used for teaching the rudiments of histology. In such a case probably nothing could be better devised than what has been done by Quekett in the first volume of his histological catalogue, or by Kölliker and other Continental authorities in their hand-books. Membranes, fibres, woody tissues, cells, cartilages, bones, pigments, silicious spicula and shell structures, are part and parcel of some organ. Wherefore disjoin them from their respective vegetable or animal organism, and render duplicates necessary when simplicity is the mother of order?

I am aware objections may be raised against departure from the ordinary routine, and the plea urged is that it is so convenient to have the elementary textures apart for separate reference. I am quite willing to acquiesce in this view if those who use it limit the application to small private collections where necessarily little more than types of fibre cell, &c., are present. But it becomes an entirely different thing its being followed out with that wonderful detail manifest in the great Quekett collection. There every variety of primary vegetable and animal tissue—cellular, vascular, and sclerous—is most elaborately represented, again to be repeated in a succeeding part of the collection.

In fine, for useful purposes, in a small as well as large collection, it seems to me a moderate illustration of elementary tissues is sufficient to be kept apart. These need not include inorganic substances, vegetable and animal components together in a series; but under each kingdom a few typical slides will convey all that is wanted concerning the material which builds up organs, &c. As to their proper position, they may precede, be dovetailed as a subsection in the midst, or terminate large groups. But I reiterate that ostensibly to anyone fairly acquaint with structural morphology, cellular, fibrous tissues, and such like, can nearly always find a convenient corner along with the group of organs they help to build up.

The microscope rigidly defines the structures differentiating the mineral, vegetable, and animal kingdoms; these groups, practically speaking, are thus sufficiently characteristic to form a groundwork of classification. They therefore serve as primary natural divisions. Proceeding from the simple to the more complex, the mineral kingdom first engages attention.

The classification of minerals and rocks is dependent either on their chemical composition or the peculiar form of their crystal—so micro-chemicals precede micro-minerals. In the structure of chemical elements and compounds, or simple and complex mineral substances, I am not aware of any gradation of minute form upon which a microscopic classification could with propriety rest. The subdivisions in either case therefore must be few, and follow conventionally the recognized systems employed in text-books.*

Amongst others, polarizing objects may form a convenient subsection. When many specimens of a single substance obtain—take, as a sample, coal sections—then a division geographically as well as structurally is valuable. Especially in such a substance as that mentioned is it legitimate, because there is no very clear gradal series to classify them with texturally. I differ from those who form a microscopic geology. Microzoal deposits or structures, whatever their nature, truly belong to the series of micro-biological specimens, if the term is applicable.

Above all, it must be remembered that the variety of purpose of a collection, its limited nature or miscellaneous character, render equivalence or diversity in grouping convenient or otherwise. But I would press my conviction, that there should always be appended to each natural division a series illustrating application to the arts and manufactures, whereby utility to every-day life, as it were, is recognized; and whereby comparison of objects as respects their purity and genuineness can freely be tested.

* I must not omit allusion to a paper by Mr. Davis Forbes, "The Microscope in Geology," 'Popular Science Review,' Oct., 1867, whose classification I adopt in my concluding section.

When the vegetable kingdom is reached, the natural orders of plants, or the divisions and subdivisions used by botanists as exhibiting natural affinities, are those which I believe should certainly regulate the arrangement of a histological collection. It may here be observed, and I should wish to lay particular stress on the circumstance, that whilst the main arrangement is a botanical one founded on natural affinities, the subsidiary divisions moreover ought to be of a physiological character. In this way, not only the differentiation of elementary structures is grouped progressively, but the separation and complexity of organic function is specially defined.

An essential difficulty in rigidly carrying out such a natural and structural classification of vegetable organisms or botanico-physiological arrangements, is the fact, that the lower forms can be viewed in their completeness in a single slide; while the higher forms, both on account of their size and segregation of organs, require many subseries to illustrate their anatomy.

This difficulty of arrangement, which equally applies to the animal kingdom, makes it essential that the lower forms, almost up to the ferns, should primarily follow their natural generic affinities, but their subsidiary divisions nevertheless be subject to physiological principles. On the contrary, the higher forms, Monocotyledons and Dicotyledons, should primarily be divided physiologically, and secondarily botanically. Further exemplified; the Root, Stem, Flower, Fruit, or organs of nutrition—skeleton, reproduction, &c., could not be arranged separately in every individual order or genera of Dicotyledons without producing a conflicting confusion in the classification.

Among the Diatomaceæ, for example, many genera may occupy only one slide. In such a case the most numerous forms or fewer leading characteristic ones, point where ought to be the true place of the slide. Again, it may so happen that a set of slides are mounted for the express purpose of demonstrating points connected with a particular locality. It would be a loss rather than a gain, therefore, to break up and distribute such a series, unless means (of which I shall hereafter mention) be taken to preserve their recognition as belonging to the original set in question. Diatomaceous earths grouped geographically or according to their geological position, stratigraphically, will teach their distribution and range in time.

Parasitical forms, such as Fungi, are usefully and naturally grouped according to the situations they are found in. But in all cases series of the genera and species should take precedence.

The changes coincident with development should form a series and subsection under the different groups.

In the division of Monocotyledons, Dicotyledons, &c., as above stated, the several organs, or apparatus, circulatory, respiratory, and such like, form the chief feature of arrangement. But fossil

forms here come to play an important part. These, when feasible, should be set under the organ and order they belong to. In the case, for instance, of stems, whether of recent or fossil woods, when the genus of the plant is unknown, a geographical division or fitness for manufacturing purposes may judiciously guide the arrangement. Fossil specimens may even be separated into a subsection where doubt exists as to their exact affinities. A series illustrating peculiarities in development, to wit, abnormalities in growth, should succeed the physiological. Vegetable fabrics, adulterations, &c., ought to terminate the vegetable kingdom.

The principles guiding the division of the animal kingdom are in many respects identical with those regulating vegetables. The ascending series is the most rational. From simplicity to complexity and the differentiation of organs, thus most naturally and justifiably are followed.

In the lower animal forms, as in the vegetable world, a slide may contain the entire organism. Here, therefore, a zoological arrangement into orders, genera, and species is sufficient. In higher forms the systems of organs, physiologically or anatomically considered, take the precedence of orders, families, genera, and species. Accessory organs connected with or subsidiary to a function follow the organ itself.

With the great bulk of Microscopic objects, generally speaking, it is not difficult to decide where each ought to be placed in a series arranged in the manner herein proposed. Nevertheless, numerous instances may occur in which it is not so easy at once to assign their position. By due consideration, however, of what special topic the slide is meant to illustrate, one may arrive at a conclusion where its nearest allies are to be found in the cabinet; and by balancing its minor relations we ultimately find a suitable situation for it.

At first sight a matter such as this may seem of trivial importance. But as the whole aim of classifying specimens is, that each should fall into its proper section, where they can be consulted almost without a moment's reflection or loss of time, it behoves that once for all a definite place should be assigned each object, where it may afterwards be readily found when wanted.

The question has been asked me under what heading ought this, that, and the other thing to be put.

Now, as I cannot predicate all the possible contingencies which are likely to arise, I must confine myself to a few examples; leaving specialities to be dealt with in a similar manner according as they may arise. For instance, where is the most fitting situation for such objects as egg-shell, coprolites, calculi, pearls, insects in amber, works of art, micrometers, and test-objects?

Treating these *seriatim*, I would say that egg-shell, although composed of the salts of lime, is virtually an animal product, and therefore to be excluded from the inorganic division. As to its physiological place, it may take rank among component elementary bodies, supposing there is such a group, either form part of an embryological section, or come under a division accessory to the generative organs of birds. Its most natural position is illustrating the textural character and peculiarities of the enveloping masses of the young of vertebrates, and therefore subsidiary to development near to the ovum.

As respects coprolites, these might either be looked upon as examples of earthy substances or as peculiar fossil forms. But if we bear in mind that coprolites are preserved as microscopical objects almost always to demonstrate the remnants of minute fossil bony structure, reptilian or otherwise, then there is no difficulty in seeing that their true position is amongst osseous structures, and along with the section which agrees in the constituent bony particles.

Calculi might with equal justice be classed with chemical substances or pathological products, and when no special series of the latter exist, the former would be quite suitable to their nature. As, however, urinary deposits commonly form a subsection of the urino-generative organs, calculi may with equal propriety be inserted after them.

Ought pearls to go along with shells or as examples of disease? This, again, depends somewhat upon the nature of the collection. Strictly speaking, pearls are but a morbid condition of the nacreous material of shells, and hence are true examples of disease. They therefore, according to principle, should be ranged among pathological textures. In a miscellaneous series, however, where pathology forms either an unimportant factor or is not meant to be exemplified at all, then pearls necessarily come under the heading Mollusca, or shell; and in a subseries either together or after the representative genus to which the shell belongs.

Insects in amber, as a rule, are kept not as typical of the structure of the amber itself, but as exemplifying a peculiar nidus wherein an insect may become imbedded. I would incline therefore to place such an object among the groups of insects. Here either close to the genus which the insect represented, or in a separate and subsidiary heading devoted to "Insects imbedded in foreign substances," at the end of insect structure.

Works of art belong to the division of "Miscellaneous objects;" micrometers to the subsidiary heading of same devoted to "Apparatus, &c." Test-objects may either be kept along with apparatus, or as I conceive quite as philosophically, with their affined group of organisms when diatoms.

I might quote many other doubtful objects, but I presume the above are sufficient to support the principle that the most pronounced feature of a specimen is that which ought to rule its position in the cabinet.

5. *Cabinets.*—On a choice of these so much depends on the intention and tastes of the individual, and the nature of the collection itself, that no rule or recommendation on my part can be given which would meet the conceptions and wishes of every amateur microscopist or professed histologist. I shall discuss the subject, nevertheless, in its broader aspect; that is to say, on such grounds as may be of general interest, or lead others to make suggestions on what at present there is no very definite standard or agreement upon.

The dimensions of a cabinet is a matter concerning economy, convenience of space, and the conception the collector intends to fulfil. If the numbers of slides are likely to be or already are very extensive, then large-sized cabinets are in some respects most advantageous. At the same time they are not free from serious drawbacks.

Among the advantages are:—

1. Stability when well made.
2. Easiness of access and reference to groups of specimens.
3. Less danger of the specimens being injured from shifting about.
4. General roominess.

Objections to them include:—

1. Expensiveness.
2. Requirements of space.
3. Awkwardness and difficulty (from weight, &c.) in their being shifted about.
4. The labour of rearrangement is great when one department becomes overstocked.

Thus it follows that according as each of these qualities has weight with those who pronounce judgment, so will the balance in favour of or the reverse be arrived at.

Smaller-sized cabinets, while deficient in solidity, roominess, &c., have still several points of recommendation.

1. They are, *cæteris paribus,* less expensive.
2. Their size is convenient, inasmuch as space and change of place or plan is involved.
3. They are more readily arranged.
4. They admit of their number being augmented agreeably to the increase of the collection.

5. If made to a uniform size and of a cubical form, they can be piled up one above the other, and so built together as to command all the advantages and none of the drawbacks of an immense single cabinet.

I cannot offer a better instance, forcing conviction of the last-mentioned proposition, than by reference to the Botanical Department and Insect Room of the British Museum. There (besides old wall-cabinets) they have a set of cubes identical in measurements, each devoted to a group or subsidiary division, numbered and labelled accordingly, and so arranged that to all intents and purposes they represent but one vast cabinet.

For study and reference they are uncommonly handy, as they can be brought down to the table, and in fact shifted about at pleasure without the slightest injury to contents.

Such is my *beau idéal* of a microscopic cabinet, compound yet harmoniously single; adapted to meet the wants of a limited, a moderate, or a numerous series; expansion being in the ratio of increment of slides. But furthermore, as I shall presently mention, the same principle is applicable to very modest microscopical collections; such, indeed, as even the amateur or those of limited means may aspire to. As a closing sentence to this clause, I may even make bold to say that, like other fashions and hobbies, that of cabinets is an infectious one: a handsome piece of furniture is attractive. Would that the zest for a thorough mastery of the contents was as powerful a stimulant.

I do not propose giving a lengthened dissertation and criticism upon every sort of cabinet, but by allusion to a few vindicate in passing the more desirable features pertaining to economy, easy access, and desirability for classific purposes.

A.—As regards space and cheapness, the common boxes with racks, sold by all microscopic object makers, are undoubtedly very handy. They are subject, however, to three great faults. 1. Many specimens, particularly those in fluid, are liable to spoil in them. 2. Reference to individual slides is awkward, from their being tilted in position. 3. Numbers and names cannot easily be read, unless by picking up one and then another, in guess-like fashion.

As an example of a considerable collection kept in the ordinary rack-boxes, I may mention Dr. Greville's specimens of Diatomaceæ in the British Museum, of which there are 3637 in all. His method of numbering and cataloguing, to which Mr. Carruthers kindly called my attention, I shall again make reference to.

B.—Dr. Müller, assistant to Prof. Hoffmeister, of Heidelberg, a few years ago kindly favoured me with a sight of their Histologico-Educational Collection. They were then adding a series of

sections illustrative of medicinal woods. In lieu of an expensive cabinet, they had adopted the following economical arrangement, whereby the slides lay flat and were easily got at.

A piece of stout millboard $7\frac{1}{4} \times 9\frac{1}{2}$ inches, and covered with coloured paper, had forty-two holes punched out. The holes drilled at equal distances, ran in parallel lines 1 inch apart. Through these an elastic cord was passed, down one hole and up the next; continuing along one line of holes and returning the next. Having reached the farther one from the point of starting, the cord is reversed and brought back hole by hole to the latter, where a knot joins the extremities of the cord. Slides are then introduced beneath the cords, which retain them in place. Each corner of the millboard, above and below, has a wedge-shaped piece glued on to it, and there is a narrow strip introduced in the middle. The trays filled above and below with slides are then piled one over the other on a shelf, or in boxes, open in front, with labels attached to each consecutive group.

I mention this inexpensive mode of forming a cabinet rather as a curiosity than to recommend it. There is one other point also to be borne in mind, *viz.* the German glass slips are shorter than the English ones, thence *multum in parvo.*

C.—Dr. Carpenter warmly commends a form of book-box as excellently adapted in lieu of an ordinary microscopical cabinet. As he says, a large histological collection can be stowed away in the library shelves, among the other books, and consulted with the greatest ease. My friend, Mr. David Forbes, has a few of such boxes slightly altered from Dr. Carpenter's plan, so as to suit the different shape of slide used by himself. The construction otherwise is similar, so that one description may suffice for both.

Each case is about $10\frac{1}{4}$ inches high, $8\frac{1}{4}$ long, and 4 inches thick in outside measurements. It opens only from behind, and has a fixed shelf across its middle. Trays of light cardboard, to the number of eighteen above and as many below, are piled on the top of each other. A small tack serves as a handle to each tray or *quasi* drawer. Mr. Forbes uses slides each $1\frac{1}{2}$ inch square, so that eight of these occupy a drawer; the number or name, according to circumstances, being towards the knob end of the drawer. Dr. Carpenter's slides are generally those in common use, 3×1 inch, and these to the same number, eight, lie athwart.

Doubtless these book-boxes are neat, and in many instances a very excellent substitute for a large cabinet. The different sets of objects are most readily classified, and the title placed on the back, apportioned to the contents. The great fault, however, lies in the one tray being so placed above the other, that to consult those which happen to be below, all the trays above must be taken out. The depth of each tray, besides, does not well admit of labelling, so that

like the rack-boxes, it is a case of trouble in searching for an object. It would be an improvement if each tray slid in on fillets, so that one might be taken out without disturbing the others, and by deepening the face labels could be placed outside. To do this would, however, spoil the compactness, and in part materially cripple their intended utility.

D.—Piper's original Portable Horizontal Slide Cabinet, as described by himself,* is composed of any number of flat cardboard trays, divided into six or more compartments, each holding a single slide in a horizontal position. The trays are enclosed in a strong millboard box, the front of which is made to fall down, so as to permit the trays to be readily withdrawn. When closed, an elastic band renders the whole firm and secure.

"It may be made of any desired capacity. Specimens are placed on the table capable of receiving from six to two hundred and fifty slides. The smallest is well adapted to contain a 'half-dozen series' of anatomical or other subjects; and its great strength, combined with lightness, makes it peculiarly available for transmission through the post.

"The one figured above (*l. c.*) is, however, that to which I would more particularly call your attention, being of a convenient size, and suitable for carrying in the pocket. It contains six trays, and will therefore hold three dozen slides.

"Among the advantages which may be derived from the cabinets, I will mention the convenience of displaying, at one view, the entire collection of slides, and the facility thus afforded for the selection of any required specimen, without the troublesome search and difficulty of removal frequently experienced with the old form of box, in which the slides are dropped (out of sight) into perpendicular grooves. It also prevents the possibility of the covers becoming detached by shaking about in transit, which is important when it is to convey a rare or valuable collection.

"The trays, being all of uniform size, may be transferred from one cabinet to another of larger or smaller dimensions, without necessitating the disturbance of the slides. In addition to its portability, it possesses the merit of cheapness, durability, and neatness of appearance."

The advantage of Piper's horizontal cases is marred by the trays resting on each other, and hence is only applicable to a very limited series of objects. The Eulenstein† collection of the British Museum, containing one hundred types of diatoms, is contained in a case after Piper's pattern, but larger, and for this purpose it answers very well.

* 'Trans. Micros. Soc. and Journ.,' 1867, vol. xv., 2nd Ser., p. 16.

† 'Diatomacerum Species Typicæ, Studiis,' Th. Eulenstein. Cent I. Stuttgartiæ, 1867.

E.—Mr. Henry George* brought before the notice of this Society a few years ago an inexpensive, compact form of store-box, wherein considerable ingenuity was displayed. The merit of his plan of store-box, or, indeed, small portable cabinet, lies in its being composed entirely of tin (japanned or otherwise), therefore of small compass, light, and not liable to warp; in the slides lying flat; and in a simple arrangement whereby the slides are kept in place without chance of overriding each other.

Each box is made to hold three or six dozen slides, or by increase of capacity to hold proportionally more. In that which contains seventy-two the outer casing is of oblong figure, $6\frac{1}{2}$ inches long, $3\frac{1}{4}$ wide, and about $2\frac{1}{2}$ inches deep. The lid is unhinged, and of ordinary form. The four sides of the box are each incised by a wide, deep semilune, so that the trays can readily be extracted. Each tray is a simple sheet of tin, out of which a large, long, oval piece has been cut, to ensure facility in taking up each slide. At the two farther extremities the tin is turned on edge, and forms a rest to the tray which lies above it. The opposite sides of the tray have their edges curvilinearly bent in, so that the slides slipping beneath are held firmly in place, alongside of each other. The slides thus lie secure, transversely to the long diameter of the box or tray, six in a row; and when one or more is wanted, by a tilting motion of the finger below the glass through the open space, extraction is easily effected.

The defects in this otherwise capital little case apply equally to those of Mr. Piper and the book-boxes, *viz.* if a specimen is wanted from the bottom row, all above have first to be removed. Again, while the labelling of each slide is readily seen on being raised, yet unless the entire contents are known, every tray has to be gone over before the thing wanted is to be found.

F.—I may refer *en passant* to Mr. Furze's zinc cases, the chief recommendation to which is their being of metal. Thus there is no liability to warp, as is also the case in the material used by Mr. George. These certainly have advantages over wood, which, unless mahogany, and that well seasoned, is so liable to warp, and render drawers stiff and troublesome to open.

G.—As a modification of the rack principle, Mr. Sorby (according to my friend Mr. David Forbes) uses a small form of box wherein the slides are ranged in rack, but instead of their lying tilted, each is placed horizontally.

A further extension of the same principle, and what are really most excellent capacious store-boxes (or to those who are satisfied with a moderate thing, a compact cabinet), cheap, portable, and

* The address of this gentleman is 65, Castle Street, Oxford Street, W. His invention has not found its way into the microscopic trade generally, but Mr. Beck, of Cornhill, showed me one specimen among his stock in trade.

each specimen of easy access, will be found in Mr. Norman's* adaptation. In a mahogany box, 7 inches high and 4½ broad, 150 slides lay in racks horizontally, and by a marginal number are easily got at and referred to. There is a diaphragm across the middle of the box, and another running up the centre longways. This gives four compartments, so many in each. A folding door at each end provides easy access, and, with a handle on the top, the box can be carried about anywhere.

H.—Mr. James Smith† has described and figured what he terms a microscopical cabinet, wherein the slides are arranged after the mode of some entomological collections. Shaped like a back-hinged book-box, on its being opened back the contained slides are all seen at a glance, being each retained in place by a double elastic band. If I am correctly informed, Prof. Hughes Bennett, of Edinburgh, uses a form of box or serial cabinet similar to the above. Other objections might be offered, but that the slides all rest vertically is against its frequent adoption.

I.—Lastly, as everything depends upon individual requirements, some wishing a small, others a larger case, it is hard to recommend one form of cabinet that will do for all. Piper's, George's, and Norman's, are each good in their way for small series, but a larger-sized cabinet of square form, with trays coming out separately, is the most preferable article. Mr. Beck sells a cheap plain kind, made of polished deal wood, wherein the trays are cardboard. In this, as in more expensive sorts, every specimen is of easy access. The latter lie flat, the numbers and name facing the observer; and labelling is provided for outside the drawer. A number of such boxes can be piled above one another, and by degrees an extensive cabinet ultimately attained.

It is now universally admitted that objects preserved in a moist medium are retained in a sound state longer and better when laid down flat. This is easily understood, for the finest and firmest cement is not always a safe protection when the slide is tilted edge upwards.

In the case of a cabinet with drawers, these are better not too deep; although some drawers of sufficient depth to admit easily the large-sized deep-celled slides are an essential desideratum. Unless in collections devoted to special subjects, deep drawers for the reception of larger objects need not be distributed throughout the cabinet; it is sufficient if they are confined to the lowermost tiers. In this way heavy specimens can be kept together, especially if exceeding in thickness the ordinary drawer's depth. The appropriate position of such objects in the general series can be replaced by a

* Manufacturer of microscopic objects, &c., 178, City Road, London, E.C.

† 'Trans. Micros. Soc. and Journ.,' 1860, vol. viii., 2nd Ser., p. 202.

dummy, or blank slide, numbered *seriatim*, and with a reference where its real counterpart is to be found. The latter, meanwhile, also bears its consecutive number in the series.

Entire sets of deep drawers having an additional shelf let in from the top I consider objectionable. These may, indeed, come in handy where a cabinet with a set of deep drawers, originally intended for another purpose, is converted into a microscopic receptacle. The microscopic cabinet of our Royal Microscopical Society has been altered in this fashion, and the available area of the double tiers consequently can contain twice the number of specimens they originally did in the deep drawers. It behoves, however, that a sufficient proportion of deep ones be retained.

The compartments of the drawers should admit of the slides lying with the narrow ends, or long diameter, fore and aft. In this way there is no chance of the slides overriding and, when the drawer is suddenly drawn out, injuring each other. In the event of a specimen, not too deep for the drawer, occupying a greater surface than the usual 3 inches by 1, provided it is not over 3 inches square, it may readily be placed transversely to the tray or drawer's direction. If it should be above 3 inches in diameter, the transverse bar or partition can be cut in such a manner that the slide shall occupy a double interspace. The end of the divided bar will prevent it moving sidewards. This simple plan, and such like trifling mechanical contrivances, are very useful in preserving unanimity in a series. They keep specimens in their proper classified position, instead of being scattered to a distance.

III.—*On Bichromatic Vision.*

By J. W. Stephenson, F.R.A.S., Treas. R.M.S., and Actuary to the Equitable Life Assurance Society.

(*Read before the* Royal Microscopical Society, *April* 3, 1872.)

It is probably well known to every Fellow of the Royal Microscopical Society that, by the aid of a double-image prism and a film of selenite, two images may be shown in the field of the microscope, the colours of which will be complementary the one to the other, and that when these images overlap, the resulting image will be, as far as the overlapping extends, of white light; but it is not, I think, so well known that when, by a suitable arrangement, different colours are made to occupy the two fields of a binocular, the resultant is a combination of such colours, and that if these are complementary to one another, the sensation of colour induced in the brain by the retina of one eye, is neutralized by that which reaches it through the instrumentality of the other, and that by the combination of the two the sensation of colour is entirely lost.

The purport of the present communication is to show how this phenomenon may readily be observed.

On the stage of the microscope before you, which I described at our last meeting, is a selenite film transmitting the green light of the third wave; this, in the normal condition of the instrument, of course fills both fields, but as the object on the present occasion is to produce different colours in the two tubes, I have introduced between the analyzing plate and *one* of the binocular prisms a film of mica of such a thickness that it will, according to the direction of its axis, accelerate or retard the transmitted ray half an undulation. By the use of this film of mica I have raised the original colour in the right-hand tube half a wave, and thus, whilst the original fine green of the third wave is transmitted to the left eye, the right is filled with the bright red higher up in the same scale. These colours are very nearly complementary, and it will be seen, on looking through the instrument with both eyes, that the field is practically white.

It will further be found that if, after looking at this colourless circle for a time, either eye be closed, a faint tint will gradually appear in the other eye, but very inferior in intensity to that which was originally experienced in looking down a single tube; and it will be observed that the diminution in intensity takes place through that eye which has continuously transmitted the same colour; although the quenching of the colour might be presumed to have ceased, when, by dropping the eyelid, the cause of its primary extinction no longer exists, this is not so; but must, to a certain extent at least, be consequent on the complementary tint having fallen on the retina of the closed eye.

The phenomenon which I have attempted to describe is not explained by the ordinary theory of stereoscopic vision, which, as given by an eminent writer, is, "that if the two images of the right and left aspects of a solid body be made to fall on the retinæ of the two eyes in such a way as to coalesce into a common image, they are judged by the mind to proceed from the single body, which alone, under ordinary circumstances, is competent to produce them."

In the case of two complementary tints viewed simultaneously through the tubes of the binocular, there is certainly a coalition of the fields into a common image; but it is clear that as no colour has been visible, the mind can have formed no *judgment* which can have conduced to such a result as we have seen is thus produced. May it not arise from the impression conveyed by the two optic nerves reaching a common nervous centre, so that the impressions from the two eyes are blended?

If over the upper half of the selenite above described a piece of mica of exactly the same thickness as that interposed between the prism and analyzing plate be cemented, but with its tension in an opposite direction, the colour of the light transmitted through the covered portion will be lowered from the green of the third to the red of the second wave, and as the upper and lower pieces of mica will (as far as they are superimposed) neutralize each other, the colours in the two tubes will be reversed; but with this difference, that whereas in the first instance we had the green and red of the third wave, we have now green of the third and red of the second, which are as before, very nearly complementary.

On examining the slide thus prepared, we shall pass, on moving it towards the observer, from fields entirely green and red, to others which are bisected by these colours and their complementary tints, rendering them respectively half green and red, and half red and green; these, again, will change on passing the slide onwards until the field which was originally green has become red, and that which was originally red has become green, but as far as colour is concerned, the brain will still remain totally insensible to the changes which have taken place.

I just now mentioned that if, under the circumstances then detailed, one eye be closed, the colour returns faintly; but it is a noteworthy fact that on doing so with party-coloured fields the colours are instantly reproduced, and, as it appears to me, with all their original intensity.

Numerous other experiments might be mentioned in which objects of divers colours appear as merely varying shades of black and white or grey when viewed in a similar manner; but to detail these would be practically a repetition of those already mentioned. Enough has, I think, been said to show how this phenomenon of "colour-blindness," if I may use the expression, may be produced.

IV.—*The supposed Fungus on Coleus Leaves; and also Notes on Podisoma fuscum and P. juniperi.*

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *April* 3, 1872.)

PLATE XVIII.

A SHORT time ago Mr. Reeves showed me a slide exhibiting a supposed fungus on a leaf of a Coleus plant, and said to grow *inside* its tissues. A brief examination led to a doubt whether the supposed fungoid objects were inside and not on the surface; and having a stock of the plants of various colours, an examination was commenced with a view to see how far the statements and opinions of two letters, written by Mr. Howse to the 'Journal of Botany,'* could be established.

It will be seen that Mr. Howse speaks of boiling the leaves in potash, and this will account for his results differing in every important particular from those described in this paper, and which were arrived at by a careful avoidance of any process that would change the character or apparent position of the leaf structure, or the supposed fungoid bodies.

In the first place a number of leaves were taken from Coleus plants of various colours, and carefully examined in their natural state, both by transmitted and reflected light. It became apparent that every leaf, whatever its age or tint, exhibited, chiefly if not entirely on the under surface, a number of globular bodies of a beautiful yellow colour, highly translucent and refractive, most of them marked with a cross like that impressed upon the well-known cross bun. These bodies differed in hue from any yellow of the leaf, and they were distributed pretty uniformly without any regard to the variegations of the leaf-colouring matter. From damaged specimens it was obvious they were the bodies alluded to by Mr. Howse. The colours of these bodies, when looking healthy, and well filled with their refractive matter, varied from rich topaz to a pale sherry tint, and they glittered like jewels when well lit up. Empty cells had a rude resemblance to a mushroom in form, with a stout stem and a round head marked with the cross, but the texture did not look in the least fungoid, nor could any mycelium be discerned in or on the leaves.

It was also found that leaves of last year's growth and those of this year had the same objects upon their under surfaces, and pretty much in the same state, except slight varieties of colour or occasional appearances of having withered. In no case out of dozens of leaves taken from many distinct plants, could anything

* Jan. 7, 1872, p. 23, and March, 1872, p. 72.

be discerned like spore formation or mycelium growth *inside* the tissues, though in one case something of the kind occurred outside.

On the 15th March a very young leaf, 5—8″ long and 3—8″ wide, was examined. This leaf was green with a dark centre. The under side was thickly set with the yellow bodies, which were distributed as freely on the green as on the dark part. The cells of the dark part when viewed by reflected light were found on the whole rounder and more prominent than those of the pale green parts. It was also noticed that the yellow bodies were not distributed at random, but ranged in something like a definite pattern with certain intervals between them. All of them were well filled, and nearly, if not quite, sessile. No stalked appearance was distinct in any leaf with the yellow bodies thoroughly filled and looking healthy.

On the 18th March, two fresh-grown young leaves were taken from another plant, the smallest being about 1—8″ long. On its under surface the yellow bodies abounded, quite as advanced in development as similar bodies on old leaves of last year's growth. Some of these contained distinct granules not unlike spores, and spore-like bodies were found on parts of the leaf. The healthy bodies were well filled and almost sessile. Dead ones showed the footstalk very plainly. On this leaf there was an appearance of mycelium, but not at all distinct.

The next observation was made on the 24th, the subject being a young leaf that had grown rapidly, and which was not quite old enough to open completely. This was carefully flattened out in a compressorium, care being taken to avoid injuring it by the pressure. Viewed by reflected light this leaf offered a most beautiful spectacle. It was full of colouring matter and fluids, and very hairy on its under surface, the hairs being developed more in proportion than the general tissue of the leaf. Amongst these hairs, glittering with fluids, some rich violet, and some white, were numbers of the yellow bodies, brimful of their coloured contents, many exhibiting the cross, and many in which it was not noticeable. They were extremely brilliant, of a fine topaz tint and high refractive power. They varied somewhat in size, a good plump one being about 1—800″, but some were smaller and some larger.

It seemed very unlikely that a quickly-grown and newly-opened leaf should have a fungoid parasite developed as well as every old leaf that was examined; and the regular arrangement of these bodies assisted the idea that they might be glandular structures belonging to the plants in a healthy and natural state. Somewhat similar bodies, but white, were found on the under surface of a leaf of garden sage; Mr. Reeves showed me others (white) on *Mentha viridis*, or garden mint; and unless it can be shown that the

yellow bodies on Coleus leaves go through some of the life processes belonging to fungi, it may be concluded that they are normal portions of the leaf structure, and not in any way parasitical. This will be the more apparent, as a note in 'Pereira's Materia Medica,' under the head of the Mint Family, speaks of similar bodies being recognized in many plants by German authorities.

If Coleus leaves are dried under moderate pressure, as between the leaves of a book, the yellow bodies shrink considerably in size, and lose some of their fine colour, but remain—at any rate for some weeks, the duration at present tested—sufficiently near their natural state to be distinctly recognized, and for their position outside the leaf to be in no doubt.

When Mr. Berkeley saw a slide prepared by Mr. Howse, he thought the bodies in question might be fungoid, and belonging to the genus *Synchytrium*. He remarked, however, that he did not know any fungus in which "the endochrome was so neatly divided." No such division could be seen in any of the leaves I examined, and it was probably caused by the potash treatment. In a recent letter to me, Mr. Berkeley expressed a doubt as to the fungoid character of these bodies; and as Coleus plants are very common, and grow vigorously in summer weather, or greenhouse temperature, Fellows of this Society can easily satisfy themselves on the subject.

Some Notes on Podisoma.—Having several specimens of the Carpet Juniper (*J. squamata*) and of the Irish Juniper—one annually infested with *P. fuscum* and the other with *P. juniperi*—observations on these peculiar fungi have been facilitated. On former occasions I have not been lucky in seeing the first form of these fungi, or what from this year's experience I take to be such. The stems they infest look gouty, with cracks in the bark, and somewhat suddenly, about April, masses of protospores crop out, yellowish brown in tint, darker in the case of *P. fuscum* and lighter in *P. juniperi communis*. *P. fuscum* forms broader masses, at first separate, but soon coalescing; while *P. juniperi* forms slender masses that swell but do not coalesce—at least that is their behaviour in the specimens to which these notes refer.

For a general account of this genus Podisoma, with the bibliography relating thereto, reference may be made to a valuable paper by Mr. Cooke in the 'Journal of the Quekett Club' (Jan., 1872). In this he says:—"If we take a portion of the orange substance which constitutes the fungoid parasite of the common juniper during the spring, and place it in a drop of water under the microscope, we shall observe that it consists of a multitude of brown bilocular spores, or spore-like bodies, with very long, transparent peduncles. These spores, for so they were long regarded, bear a striking resemblance to those of some species of Puccinia, with this difference, that they are imbedded in gelatine, whereas in Puccinia the clusters

of spores burst through the cuticle of the plant on which they grow, free of each other, and when more compact than usual not held together by a gelatinous medium."

This year, being on the look-out for the first outburst of Podisoma, it was noticed on the 29th February, on a Carpet Juniper not in a particularly warm situation, but the temperature was, as is well known, above the average of that month. A note made at the time says:—"Immense numbers of red or red-brown protospores burst through the bark in patches, each spore detached; no gelatinous enveloping mass. With 2—3″ objective looked like a multitude of garnet beads; with 1—5″ the two-celled character of each protospore and its granular contents became distinct." The protospores varied in size, and many of them differed somewhat from the typical form. Some were more elongated, others more squat; some had little nipples at the top, and others had smoothly-rounded tops. Amongst them were found a few trilocular cells, and one delicate crescentic spore-case with about twelve divisions.

Portions of this Podisoma, with the bark of the wood on which it grew, were placed on parchment paper and moistened. In a few days some jelly was formed, but further development did not occur, and it was soon covered with a white mould.

On the 2nd March the Juniper Podisoma was first seen on Irish juniper trees, without any gelatine. The outcrop was in much smaller patches than that of *P. fuscum* on the Carpet Juniper, and had the aspect and feel of little tufts of dry sponge. The biggest of this date were not more than 1—8″ long. Viewed as an opaque object, these tufts looked like aggregations of brown spun sugar threads; those on the surface projecting, each one distinct from its neighbour, the rest adhering close together, but no sign of jelly. With sufficient magnification the structure was seen, a two-celled protospore growing at the end of each thread. Portions were squeezed in a compressorium, and a multitudinous entanglement of mycelium threads disclosed, on which the protospores grew. The appearance of *P. juniperi* in this stage was not at all like the garnet-bead aspect of *P. fuscum*. Some multilocular cells were mixed with the characteristic cells of this fungus. Many of the protospores at this stage differed more or less from the normal forms.

By the 18th March *P. fuscum* had formed a good deal of gelatinous matter on Carpet Juniper, and a little on the Irish Juniper was formed by *P. juniperi*. By the 29th the fungi on Carpet Juniper had passed the stage of distinct development of each gelatinous mass; adjacent masses had become confluent, and all were in a soft, pappy state. Portions of this jelly showed under the microscope that great numbers of the protospores had germinated, and this had taken place with more irregularity of detail

than last year. Some protospores had four tubular projections, two from each side, each one springing from its own side of a cell division, and not always from exactly the same part. Some had only put forth little roundish vesicles, and many only developed tubes on one side, some only a single tube, the rest being abortive.

At this date (April 1) the Irish junipers show their fungi in a much less developed stage.

Mr. Reeves mentioned to me a case of *P. juniperi* that had formed large warty excrescences on a tree belonging to Mr. Wollaston, of Chiselhurst, and that gentleman has kindly forwarded me a sketch. It appears that the juniper on which this fungus grows had two leaders, and Mr. Wollaston cut one close off, leaving the usual cicatrix. On each side of this cicatrix the fungus grows, the gelatinous tufts springing annually from two bumps which the fungus has formed. Can this be regarded as an approximation to the habit of the American *P. macropus*, which makes peculiar galls, that surround the stems it affects, and which have pitted facets from which the spore masses spring? The occurrence of *P. fuscum* and *P. juniperi communis* in a non-gelatinous stage assimilates them to *Puccinia gymnosporangium*, which Mr. Berkeley informs me is separated from Podisoma by the gelatinous element of the latter.

EXPLANATION OF PLATE XVIII.

FIG. 1.—*Juniperus communis*; the swellings on each side towards the base produced by *Podisoma juniperi communis*. (Mr. Wollaston.)
,, 2.—*Podisoma juniperi communis* growing out of the swellings in Fig. 1.
,, 3.—Kind of gall produced by the American *Podisoma macropus*. The fungus grows out of the facets, as in Fig. 3—n. s.
,, 4.—Group of protospores of *Podisoma juniperi communis* bursting through cuticle, before formation of gelatinous matter × 135.
,, 5.—Protospores of ditto × 240, with mycelium stalks.
,, 6.—Group of protospores of *Podisoma fuscum* bursting through cuticle, before formation of gelatinous matter × 135.
,, 7.—Protospores, ditto × 240, with mycelium stalks.
,, 8.—*P. fuscum*—n. s.
,, 9.—Portion of very young Coleus leaf, with hairs and glands (yellow) having a cross mark upon them × 135.
,, 10.—Lobe of larger leaf, showing the arrangement of the yellow bodies × 60.

V.—*Optical Curiosities of Literature.*

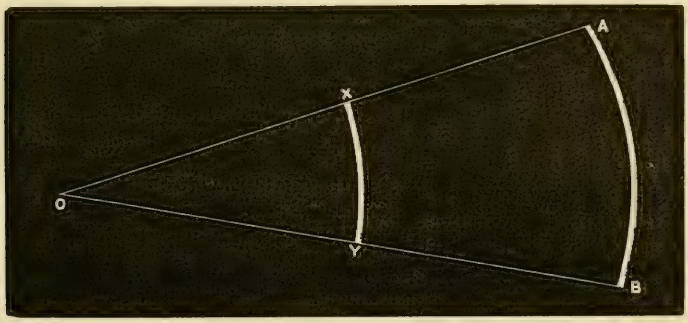
By the Rev. S. Leslie Brakey, M.A.

There is no class of artists which receives so large a share of respect and admiration from their employers as the professional makers of microscopes. Those who use these instruments are, above all men, interested in the excellence of the performance of which they are capable. They are incessantly examining and testing them, comparing their glasses with their neighbours' glasses, or the glasses of one maker with those of another maker. But the qualities they are in search of depend upon conditions which are, for the most part, entirely out of their reach. They can recognize the finest shades of performance when they see them, but are wholly ignorant of what they depend upon. As a necessary consequence the makers, who hold in their hands the secrets of these things, are looked up to with that peculiar awe which always attaches to the mysterious and unknown. With many workers this pleasurable emotion lasts to the end. I confess that, when fresh to the work, I have to some extent shared this feeling; but, less fortunate than some others, have experienced the painful sensation of having my eyes opened by degrees, and opened at last very widely indeed. Having been drawn on, as all workers are who really love their work, to follow out some special lines of investigation, and so to become dissatisfied with my apparatus, I used to make occasional visits to London for the purpose of having it modified. In course of this work I visited a great many different "houses." Acutely conscious of ignorance on various points concerning the structure of object-glasses, on which neither published books nor mathematical calculation could throw light, I endeavoured to "improve" these occasions by conversation, on the chance of picking up something which might throw light on the secrets of the business. On what curves depended flatness of field,—what was the difference in effect of single and triple fronts,—what was the connection between "actual" focus and angle of aperture,—these and such like were the things I sought to find out.

It was this spirit of inquiry which led to the disenchantment I have spoken of. Things occurred occasionally which began to raise my suspicions that opticians did not always themselves know what they were doing. Of my earlier experiences here is an example. Having some alteration to be made in a stand-condenser, an optician, well known to fame, incidentally observed that the glass was wrongly made. It was double-convex, whereas such a condenser "ought always to be plano-convex." It was quite essential; and he spoke with much severity of the carelessness of makers who used the wrong construction. I assented, in theory at least; and as he

offered to change the glass, a plano-convex was then and there fitted on. Presently observing, to my surprise, that in trying something, he had turned the round side to the object, I asked if he had any reason for preferring that side to the other. To this he replied that—yes—it certainly would work with the other side just as well; either side would do; there was no difference in that way, only it *must* be a plano-convex.

It was something of a shock to me to find that an optician of no obscure name did not know the meaning of a plano-convex lens; but this was only a beginning. After some more incidents of the same kind, I was destined to receive another shock very much more severe. In conversation with the head of another house, I happened to remark on the smallness of the mirror in a microscope that was standing near. The optician replied that—yes—it *was* small, unusually small perhaps, but—it came to the same thing in the end. The fact is, he proceeded to inform me confidentially, there was a great misapprehension about the size of mirrors: few—*very* few, were aware of a fact of which he had satisfied himself by demonstration, that the illumination was exactly the same, whether the mirror was smaller or larger. I expressed anxiety to know something more of it; and, calling for pen and paper, he drew for me the following figure:—



"Now," he said, "you see here is the large mirror A B sending in a pencil of light to the point O. Very well; now suppose a smaller mirror placed in this way, as X Y, you see how it comes to just the same thing; stands in exactly the same angle of light, and so sends in the same pencil."

With this the last remnants of my faith passed away, and I ceased to be surprised at anything. I might add some more curiosities of literature from my experience to these two, which were the first and the last; but their interest would depend on the names of the opticians concerned, which, for obvious reasons, could not be given. Those concerned in these cases were names perfectly well

known and familiar, though not in the very first rank. For, I ought to add, none of these incidents occurred in connection with any of the three historic great houses of which England is so justly proud, with all of which, so far as my experience has enabled me to judge, optics means something more than mechanical work.

The anomaly which so much perplexed me when new to the work, I believe that I can now perfectly account for. Optics, in a special sense, involves both principle and practice, the combination of the two being necessary at almost every step, so much so, that this department of knowledge cannot with propriety be called either an art or a science. But in certain cases the scientific part is reducible to fixed measurement and figures, and in this form can be handed over to the workshop. *Some* opticians have in this way acquired not the Science but the Art, working under others from whom they have received the figures; afterwards working the same figures on their own account. Their glasses are thus made *to formulæ*, the makers remaining in total ignorance of the "why" and the "wherefore" in which the formulæ originated. They work to these, and within certain narrow limits work well. It is only when something new is required, some modification of existing designs, that the deficiency is made apparent, and they are, in a literal sense, thrown out of their reckoning.

The curiosities I have been speaking of are not, however, by any means confined to the literature of conversation. Feats of learning which may well bear comparison with them are to be found in published works—works which have a name and a certain measure of authority. I will take an example from the treatise of Dr. Lardner in the 'Museum of Art and Science.' In vol. vi. the question of Nobert's lines is taken up, in connection with the reports of the juries of the Exhibition of 1851. The juries, it seems, had reported that while certain moderate powers sufficed to resolve the lower bands, to resolve the upper ones powers not only higher, but very much higher, were found to be necessary. Here the writer's calculating power discovers "a mistake." He counts the bands so as to find the proportion which the closeness of one band bears to the closeness of another. The higher band is, suppose, five times as close; therefore, a magnifying power exactly five times greater must, he says, resolve the higher band. For is it not plain, demonstrable mathematically, that this will present the lines so as to subtend the same angle which in the first band was found sufficient for their separation? And in fact, says the author, proceeding to generalize, we never need consult our microscopes at all to know what amplification will separate a given band. Ask how many lines it has to the inch, take the proportion of this to any band already resolved, and you have the required amplification at once; a simple sum in Rule of Three. Finally, the writer expresses him-

self as painfully surprised that the jury should have overlooked this short and easy method, and so fallen into the mistake; and is the more anxious to call attention to it at once, as he finds that unhappily the "mistake" has got copied into some microscopical treatises.

It would be hard to find now even a beginner who could read this passage without smiling. No doubt we are more familiar with these famous lines than observers were then; but allowing for this, and passing over any amount of contempt for the eyes of the jury, it still seems unaccountable that to one familiar with scientific methods, the possibility never suggested itself that the different conditions of microscopical from ordinary vision *might* introduce something which should modify the abstract calculation. But the possibility, if it ever suggested itself, seemed so remote as not to make it worth while even to take down the microscope to look.

Practical errors, however, are not the only errors in this treatise. There are others, which if less amusing are more discreditable, being things of pure science. Take, for example, the passage in vol. viii., where the author—perhaps someone else, and not Dr. Lardner himself—undertakes to show the *rationale* of the magnifying property of a simple lens. He premises the discussion by laying it down as a thing of primary importance, and specially so because there is "no subject on which more inexact and erroneous notions prevail." Having sounded this flourish, he proceeds himself to explain it, and explains it—*wrong*. An object brought nearer subtends a larger angle, and the lens enables the eye to see it when brought nearer, nearer than the customary ten inches;—this is what the explanation comes to. If the writer had ever happened to take out a pocket lens while reading and held it before the book, he might have observed that the print was magnified, although the distance remained the same; and might thus have been led to avoid the fate of himself increasing the list of "inexact and erroneous notions." Some more examples of the same kind may be found in vol. ix.

Not to go farther away, a number of remarkable flowers might be culled from the pages of the 'Monthly Microscopical Journal,' even at this early period of its youth. One of them will be found in a paper by Mr. Mayall on immersion objectives, in the number for February, 1869. Mr. M., who appears to have had an unusually extensive acquaintance with glasses of this kind, observes, as it were in passing, that in consequence of the refractive power of water an immersion glass never "has to do" with rays of greater obliquity than 48°. This observation, which is made with the easy air of feeling perfectly at home in the subject, has evidently been a source of much disquietude to readers of the article in after-times. Some correspondents manifestly had become convinced that there must be a mistake somewhere, though not able to say exactly where or

why. It was not perhaps to be much wondered at that an amateur worker should himself have made a mistake like this, or even that he should assert it in such a self-complacent tone. The unaccountable thing is that he should have passed over the startling discrepancy between his own doctrine and the professions of the makers he was eulogizing. For in the lists of Merz, Hartnach, and Gundlach, with all of which he is on familiar terms, the immersion apertures are given rising by degrees to 175°. But whatever may have been the origin or account of his error, Mr. Mayall has himself preserved ever after a judicious and most commendable reticence, following in this the well-known wisdom of the "leading journal," which, once committed to a misstatement, neither withdraws nor explains, leaving its readers to explain it for themselves as best they can.

But the choicest of the flowers by many degrees is that which was presented by Mr. Tolles, of Boston. The episode is too recent to have been forgotten, but, the discussion being ended, it may be regarded as now in some sort appertaining to history. It was on this wise. The original controversy about angular aperture having some time before come to a close, Mr. Tolles who, unknown to everyone, had been studying it, appears unexpectedly with a contribution which seemed to open up an entirely new phase in the question. Putting theory aside, he simply tests the fact by an experiment. And he finds that the disputed aperture *can* exceed the famous 82°, because in fact it does exceed it, being 110°. This certainly seemed startling, for facts are proverbially hard things to reason against, and Mr. Tolles, a practical worker, might be supposed to know a fact when he saw it. It appeared, however, that this was just what he did not know. Mr. Wenham instantly pointed out the fallacy; he had made a mistake in arranging the saddles for his horses, or, not to use metaphor, had *measured the wrong ray*.

So far the oversight was not unnatural, although one which a "professional" might have been expected to look at twice before committing his reputation by printing it. But a greater thing was coming. Having again reflected for some time, Mr. Tolles again appears, and this time also on a new ground. The experimental line having been found not to work, he now elects to win with Theory. Cementing to the front of his object-glass an imaginary hemisphere to match it, he forms an imaginary sphere, and then proceeds to reason upon it. As thus:—The rays can pass into this sphere below, and out of it above at every angle. And there is no difference if the hemispheres are connected, not immediately, but with the slide and immersion water between them. As before, the rays will pass in and pass out at every angle, and so the aperture has no limits at all.

This is the flower I have spoken of as being, all things considered, the most remarkable yet seen. You cannot, says the proverb,

have it in meal and also in malt. Mr. Tolles, however, having taken a good while to think of it, imagined that he could. The front of his object-glass he uses to pass the rays straight on, the refraction being neutralized with fluid. Then, oblivious of having used it up, he all the same expects the benefit of its action the other way, as a refracting surface collecting the pencil for the eye-piece.

I think that this may fairly pair off with the discovery of the man with the diagram of the great and small mirrors.

Mr. T.'s ingenious idea was disposed of by Mr. Wenham in a single line, by asking the question, "Do you expect to collect your whole aperture with the back combinations?" And after this it might have been supposed that Mr. T., in silence and meditation, had learned at length to avoid the pitfalls of this question. But from an incident which has since occurred I gather, with regret, that he is still in difficulties. A letter addressed by me to Col. Woodward, under the signature "B," concerning the aperture of his $\frac{1}{5}$, Mr. Stodder volunteered to answer (Jan., 1872). He replies for the four cases, immersed and dry, uncover'd and cover'd. In three of the cases the figures are no doubt correct; in the remaining one—the object immersed, uncover'd—prompted by Mr. Tolles he reports the aperture as 140°!!! Col. Woodward, who also courteously replied, also answers for the four cases; but, better instructed than his countrymen, on coming to the critical case he leaves out the figure, restricting himself to saying that, after a certain turn of the screw, the glass may now be used for objects immersed uncover'd.

I pass to notice some other passages remarkable in different ways, but not for the same kind of reasons as these. I insert them here for convenience only, and in no sense intending to class them with those already given.

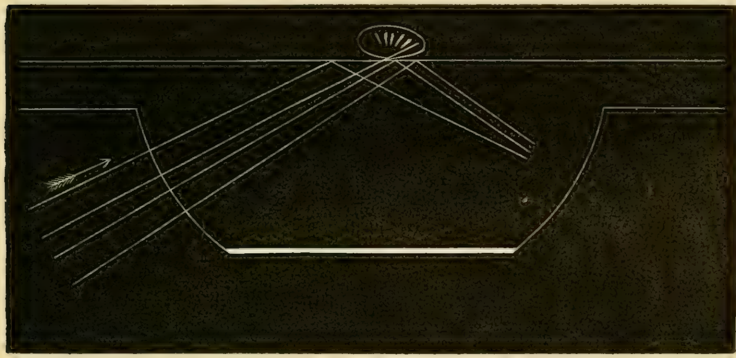
In a paper by the late President, the Rev. J. B. Reade, a sentence occurs which seems difficult to account for. Experimenting with his prism, he announces* that when it is turned so as to reflect light to the tube from its upper plane face the beam will be found to be polarized. This he gives as a fact so novel and startling, that he even anticipates it will be received with incredulity—"as a fable." Can it be that he was till then not aware that at a certain angle light is polarized by reflexion? He was before my time; but I have always understood that his scientific attainments, more especially in optics, were of a very high order. Yet in this passage he speaks precisely as Malus might have spoken on that evening when, sixty years ago, to his own astonishment he "vanished" the sun's reflexion from the windows of the Luxembourg with his doubly-refracting prism. It may be, however, and I should be glad if it can be shown, that the passage was meant to bear some other meaning, and that the mistake is mine.

* Vol. ii., p. 83.

In August, 1871, a communication appears from M. Mouchet on the thickness of glass covers. He there describes his method of measuring it, *viz.* by focussing for the upper and then for the lower surface, and observing how far the tube has been lowered, ascertainable by the index of the fine-adjustment screw. This method of taking soundings, as it might be called, is, I believe, discovered independently and used on all occasions by almost everyone who works intelligently with the microscope. It suggests itself, indeed, or rather forces itself on our notice every time we begin to work the screw. It is not, however, for this reason that I notice it, but to point out that M. Mouchet has by an oversight recommended an erroneous measurement with it. The distance traversed by the microscope is not the thickness of the glass. We must in every case correct this distance by increasing it in the proportion of three to two, due to the index of the glass. Of course so long as we only want a correction for the screw-collar the error is of no consequence; the proportion of this adjustment to the index of the slow-motion screw remains the same. But if we go outside this, applying it *e. g.* to the measurement given on some slides by the preparers, in absolute fractions of an inch, then the error will come out: as also when we use it, like M. Mouchet, to measure the absolute thickness of diatoms in balsam.

In the report of the proceedings of the Royal Microscopical Society, in the number for December, 1871, I notice observations of Mr. Slack and Mr. Brooke on the new lens introduced by Mr. Wenham, which do not correctly state its principle of action. There are two modes of action of which this lens is capable, and these in their nature and conditions are entirely distinct. In the observations referred to (p. 294) these two are mixed up together, so that the meaning of the illumination is lost. This may possibly be due to the necessary compression of the report, the observations having been made in conversation. Taking them, however, as they appear printed, they are not correct, and as they remain still uncorrected I notice them; principally, however, for the purpose of calling attention to the action, as yet it would seem so little known, of this most peculiar illumination. The persevering efforts which for some time have been made, as yet with no sign of success, to make out disputed structures with very high powers, are forcing us to pause and review our resources. We have in fact come nearly to a standstill, and the indications are, I think, becoming very plain that we can scarcely hope to "force the game," if I may so express it, by increase of power, or even by fineness of corrections alone. For in cases where, the definition being good, the magnification appears amply sufficient, the structure still refuses to reveal itself, and we come to a point where increased amplification yields no increase of knowledge. I have been for some time becoming more and more convinced that we must turn

our attention very much more than has yet been done to illumination *and its interpretation.* Things seen with high powers and large apertures are seen under conditions essentially differing from those of ordinary vision; so that in these realms it ceases to be true that seeing is believing. And when the best has been done for our glasses, we must still, I fear, supplement the discernment of our eyes very largely by the discernment of our reason And this of course involves chiefly the question of illumination and its illusions. At present the methods we possess are three—opaque, direct-transmitted, and dark-field. The lens in question has now added another which in nature and principle differs from all these, and supplies us with what I may call a new analysis of the objects as seen. But to use it so as to make it an analysis it is even more necessary than in the other methods that we understand the principle on which it works. I therefore append a diagram, which may



help to effect this by showing the action reduced to its most simple form. The lens and slide are here represented as forming a single piece, which optically they do (from the intervening oil). The object rests on the slide, touching it here only at a single point. The cover is omitted intentionally, having nothing at all to do with the work. It may no doubt be placed over the object, it is of no consequence at all whether or not; provided always that if placed no fluid is added between, and the object must still be, as here, on the slide. For simplicity therefore I omit it entirely. All the light which enters the slide is lost by total reflexion, except the ray which reaches the single point of contact. This ray is transmitted by entering within the object itself, and is there dissipated, more or less, according to its structure. This method therefore is distinct in nature from those before in use. Mr. Wenham has given no name to it. I have been accustomed to call it *Internal Illumination.* It is seen at once that it gives an analysis of the contact of the object with the slide. It might be thought the illumination would be symme-

tric, the field equably lighted; but this is not altogether so. It is so, however, to a greater degree than in any other illumination; and I have used it for the Binocular with an $\frac{1}{8}$th of large aperture with perfect stereoscopic effect. This, however, is not at all times very easy, the direction of the light sometimes making itself very evident; and I confess that I cannot as yet always control its action so as to reproduce effects before obtained. Moreover, in applying it to very fine objects, like Podura scales, where there is in parts almost, though not absolutely, contact with the glass, difficulties begin to appear, of theory too as well as of practice. The limits of the dissipation of the inner light have to be considered; and indications show themselves of conditions limiting the absolute inflexibility of the law of total reflexion, which require for their elucidation more knowledge of the laws of light than any student of optics, I fear, as yet possesses.

The other action of this lens I mention only to exclude it. It works with media only; the upper surface of the cover then reflecting down the light. It is not therefore new in its nature, but only an opaque illumination by a special method. It is also, to my experience, very hard to manage; and I do not expect much can be got from it. At any rate it has nothing to do with the diagram given above, or the remarks I have made upon it.

The preceding paper was written under the impression that the discussion in which Mr. Tolles was concerned had ended. From the March number of the Journal, received this morning, I find that this supposition was premature. Not dismayed by his two falls, he puts in yet another appearance, or rather two appearances. If Mr. Tolles has any enemies, which I hope he has not, they must rejoice that their enemy has written, not indeed a book, but four scientific papers. In these last two he has, as reviewers say of the latest work of a novelist, surpassed himself. It is in truth scarcely possible to discuss seriously the absurdities of these articles. That Mr. Tolles meant them seriously, there can, I think, be no doubt; but if he had meant them as a jest, a more prosperous one was never sent out. Changing his front once more, he thinks he now really at last has caught his aperture. In his own language he has designs, two at least, for its "procurement." The back combinations having failed him, he now has remedied the defect (on paper). His objects he will mount in the insides of little glass balls or *pillulæ*, which shall be quite separate from the object-glass. Then when the rays come out as before at every angle, he will look into these little balls with a complete object-glass, a glass with its own front, a glass of "three systems"—and so the thing is, as he says, "ended." We shall see. Suppose—it is an impossibility, a twofold

impossibility, but suppositions cost nothing—suppose a high-power glass could be made to reach the centres of the *pillulæ*, what becomes of the immersion? For to command the aperture, the glass must work *dry*. And if, changing it yet again, he designs another remedy by applying what he calls the "flowed-in-liquid," what becomes of the aperture? for as soon as the liquid is "flowed-in" the aperture is flowed out, and the inexorable 82° is upon him again.

The rest is in keeping. The best thing is where he quotes the experiment with the water-tank against itself. A $\frac{1}{12}$th in the tank showed 100°; and this being admitted he cannot for his life think why it does not end the case; for, as with touching simplicity he observes, 100° is surely larger than 82°. So it seems that after having had just half a year to think over the experiment, Mr. Tolles has never found out that in this case the object is in water as well as the objective; and that the same law which admits the higher limit for water, necessitates for the very same reason the lower limit for balsam.

In his new curves which form additional designs he is equally at fault. His diagrams are falsely drawn, and the lines do not give the refractions they are indicated as giving—a grave fault at any time in a professional workman, but altogether inexcusable where the very point at issue turns on the magnitude of the angles in question. Into this I need not enter, however, in detail; for though in one sense public property, it is in the first place the business of Mr. Wenham, who is the most competent to show its merits. Whether he will choose to do so is of course another question. Having twice set Mr. Tolles on his feet, he may not feel himself called on to repeat the process indefinitely. In any case I should not, for the present at least, have noticed it, but for the accidental coincidence of its coming in connection with the paper just written; supplying as it does a new and unexpected illustration of the extent to which skill of a high order in manual work may coexist with absolute ignorance of the principles on which the work depends.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Subject of Binocular Vision is partly a microscopic one. We therefore notice a paper which we had intended to have reproduced, but which we now for various reasons are compelled to give up. It is by Mr. Joseph Le-Conte, Professor of Natural History in the University of California, and is really a very interesting communication. It enters upon a discussion of M. Pictet's views, which it disputes, and the author supports his views by some very ingenious experiments. The paper is of considerable length, and has been published in two or more numbers of 'Silliman's American Journal of Science,' to which we must refer our readers for further particulars. —*Vide Silliman's American Journal*, December.

The Development of Microzymas.—A claim to priority of discovery of the development of these organisms has been made to the French Academy.* M. Bechamp asserts, it is alleged, that the mycrozymes or Bacteria unite together, and thus form a cell. Now M. de Seynes asserts that this fact was pointed out by M. Pineau in 1845. In the number of 'Comptes Rendus' referred to, M. Bechamp says that he never asserted that the bacterias or mycrozymes unite to form a cell; on the contrary, he pointed out where they may become converted into Bacteria. He admits that Henle's Anatomy (1843) does contain some ideas like his, but they were put forward as mere speculations.

Spontaneous Generation.—On this subject, which has of late, through Dr. Bastian's publications, received so much attention, two lectures have been delivered in New York before the College of Physicians and Surgeons, by Professor J. C. Dalton, M.D. These give a useful summary of the various experiments that have been tried in the several European countries and in America during the last two hundred years. The lectures are full of interest, and extend over forty pages of the 'New York Medical Journal.' † We therefore commend its consideration to our readers. The author seems more opposed to spontaneous generation than in favour of it, as may be seen by the following concluding remarks:—"Thus," he says, "we find that now, as always, the idea of the spontaneous generation of living beings is confined to organisms of which we know the least. Exactly where our definite knowledge fails, owing either to the minute size or the imperfect organization of these bodies, there commences the obscurity which hangs around their origin. It is very justly said, in support of their spontaneous generation, that, if this mode of production exists at all, it is precisely in the case of the simplest and most imperfect organisms that we should expect it. We might imagine a bacterium or a monad to originate in this way, but not an eagle or an elephant. On the other hand, it is alleged that the imperfect organization of these minute forms is only apparent, and depends on the imperfection in our means of observation. When our microscopes and other aids to investigation have been still further improved, we shall find, it is said, that the bacterium and the vibrio possess an organization of their own, not less essential and complete in its way than that which we

* 'Comptes Rendus,' Feb. 19, 1872.

† February, 1872.

now know belongs to the ciliated infusoria. There is every evidence that at least their regular and normal mode of production is from germs disseminated in the atmosphere; and they themselves, as we have already seen, are embryonic or transitional forms in the development of a distinct vegetable growth. They are consequently to be regarded as an integral part of the cryptogamic vegetable organizations; and, notwithstanding the apparent simplicity of their structure, they no doubt, like other plants and animals, have their definite place in the organic world."

NOTES AND MEMORANDA.

Papers "standing over."—Owing to the pressure on our space, we regret being compelled to allow papers by the following authors to stand over:—Dr. Lionel Beale; Dr. Braithwaite; Dr. R. H. Ward; Captain Hutton; Mr. Isaac Roberts; and Mr. P. Braham.

"Pattern Lead" for making Cells.—Mr. White, the secretary of the Quekett Club, and one of the members of our Council, stated at a recent meeting of the club that for many years he had been in the habit of using cells made of a thin kind of lead known as "pattern lead," which was used by dentists for taking patterns for their gold plates. It would be found to answer the purpose very well, and had none of the objectionable qualities mentioned by Dr. Matthews (in his observations at same meeting), since the slide might be made almost red hot without melting the cells, and the cells were very easily stuck on with marine glue. For shallow cells a simple ring of gold size, and gum dammar put on thickly and allowed to get hard, answered the purpose very well, and if Bastian's cement were used instead, the cell could easily be built up higher by adding layers upon those which had become dry. Another way was to use the zinc cells, which would stand any amount of heat; acid, however, would affect these, but vulcanite cells would resist acids. In making cells for mounting in fluid, it would be found of great advantage to set up some standard size, and keep to it, as this would enable the worker in a short time to estimate correctly the exact amount of fluid required for filling—a matter of very much importance.

Colonel Woodward's Paper in the April Number on Tolles' Objective $\frac{1}{16}$th Immersion.—Colonel Woodward has requested us to add the following observation to his last paper, which, however, has already appeared in print:—"Since writing the above paper I have made photographs of the two frustules shown by the Wales' lens, with a lens of Mr. Tolles' (a $\frac{1}{16}$th immersion), which I think fully equal to that by the Wales' objective. I may add that any of these objectives, including the Becks' $\frac{1}{10}$th, will resolve *Amphipleura pellucida* in balsam, as, in fact, was done by Count Castracane with objectives by Hartnack and Nachet.* Count Castracane made photographs of the balsam-mounted specimen on Möller's type plate, and counted the striæ."

* See this Journal, April, 1871, p. 176.

CORRESPONDENCE.

The 'American Naturalist' and Mr. Wenham.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Seeing in page 178 of last Journal your notice quoted from the 'American Naturalist,' I ask for a brief reply—not in the way of controversy, for the insignificance of the case does not call for this, and the writer, C. S., may remain under cover of initials, but merely to correct a misstatement, that I wrote "a paper in reply to one of Mr. Bicknell's." I did not commit myself to such an extent; all that I said in reference to him was included in *one paragraph of fifteen lines* * as a protest at what I called an uncourteous remark against Colonel Dr. Woodward, whose name is now familiar to microscopists for his valuable and practical discoveries in a most important and interesting department of the science.

Accusing Colonel Woodward of wilful deception, and Messrs. Powell and Lealand inclusive, I think is generally to be admitted as something more than uncourteous, and so near to an *unwarrantable personal insult*, that it would have come with a far better grace if they (C. S. and Mr. Bicknell) had given some plea or atonement for expressions, if hastily or inconsiderately written, than to expect *me* to "apologize" for what they term "practically defending the imposition"!! No one knows better than myself the difficulty of adopting a nomenclature by the different makers that shall exactly denote the power of all the highest object-glasses sent out. Every user must do this for himself, and so it will ever remain; and as one witness on behalf of at least the most respectable portion of the body, and holding in scorn the abuse of some whose opinion is evidently not worth caring for, I finally venture to affirm that no imposition is intended.

Yours faithfully,

F. H. Wenham.

PROCEEDINGS OF SOCIETIES.†

Royal Microscopical Society.

King's College, *April* 3, 1872.

Dr. Millar in the chair.

The minutes of the previous meeting were read and confirmed.

A list of donations to the library and cabinet was read, and a vote of thanks passed to the respective donors.

Mr. Stephenson read a paper "On Bichromatic Vision."

Dr. Pigott was much delighted with Mr. Stephenson's description

* *Vide* page 292, 'M. M. J.,' Dec., 1871.

† Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

of this very subtle process of regulating the thickness of the waves of light as applied to the binocular. He thought Mr. Stephenson had very perspicuously explained the action of the coloured fields which he employed. He (Dr. Pigott) had not tried any experiments of the kind, but the ideas set forth by Mr. Stephenson were very suggestive.

After referring to the effect produced when one is looking through a pair of spectacles, and the two rims are noticed or not, Dr. Pigott continued: Suppose that the two glasses were of different colours, what would be the effect upon the eye and brain? He imagined that if they were complementary colours, they would be united by the brain, and that if we were looking at snow we ought to see it tolerably white. He thought that Mr. Stephenson had touched upon a new point in optical science, which might be developed with very great utility. The ingenuity of the whole thing struck him with admiration. Looking at the very complicated manner in which rays of light are refracted and reflected by internal reflexion, and according to the position of the prism, it was wonderful that such good effects were obtained.

Mr. Slack said there was one point in Mr. Stephenson's paper which seemed to require explanation. He referred to the portion of it which treated the effects described as having a character distinct from stereoscopic binocular vision, which it really resembled very closely. Mr. Stephenson explained that the remark alluded to was not his own, but a quotation. Mr. Slack proceeded to point out the resemblance between the combination of two perspective views, and two complementary colours. In both cases it seemed that the perceptive portions of the brain acted upon the combined images.

The Chairman said the interesting point in Mr. Stephenson's paper was that in which he referred to the fact that in each retina there was a different colour, and yet it was seen combined into one colour. He did not remember in any work the subject being treated in the same way as Mr. Stephenson had treated it.

Mr. Slack said he should like to suggest an experiment, *viz.* to try with all sorts of persons. It was known that there were very few people who had two eyes exactly focussing alike, and probably it would be rare to find a pair of eyes in which the chromatic corrections were the same. It would be very interesting to notice what would be the different effects the same images produced upon persons whose eyes had different corrections.

Dr. Pigott said he had met with the case of a gentleman who, looking through his microscope, said he could never distinguish a *red* colour in the instrument. It always appeared to him more like black and white. He never saw anything *red*, but objects that appeared *red* to other persons were black to him.

Mr. Ingpen referred to an experiment tried with the stereoscope, in which tinted glasses were used, having complementary colours. The result was sometimes combination of colours, and not unfrequently alternation of colours. The alternation might be attributed to the fact that the eyes got alternately fatigued by each colour.

Mr. Slack read a paper "On the Supposed Fungus on *Coleus* leaves," and "Notes on *Podisoma fuscum* and *P. juniperi.*"

Mr. Howse said, having re-examined many specimens, he was now quite of Mr. Slack's opinion in reference to the supposed fungus.

Mr. Slack said he had suggested that probably the treatment with potash was the reason that there was the fungoid appearance. The best way to view such things is in their natural state.

Mr. Howse had treated them with potash that he might see whether the bodies were outside or inside; they seemed below the cuticle.

Mr. Slack said the little globes are above the cuticle in its natural state, but if the cuticle was swollen by chemical means they might appear inside.

Dr. Braithwaite thought that Mr. Slack was quite correct in the inference he had drawn. The Coleus plants expelled a fœtid odour something like the Lamium, and the yellow glands might evolve it.

Mr. Slack said in reference to Podisoma he had noticed a curious appearance in one of his slides, in which he had put by a portion of the gelatinous matter that he had alluded to without any preparation whatever, just to keep it free from dust in a perfectly natural state; when it began soon to develop one of the moulds. The same form of mould grew upon a specimen which he had kept upon a parchment paper and rice paste. It exhibited little black globes at the tips of white threads. When the black globes were squeezed they broke up into a number of little spherules which acted as spherical lenses. When aggregated together, these highly-refractive bodies presented an appearance like a black pin's head.

The meeting then adjourned to the 1st of May.

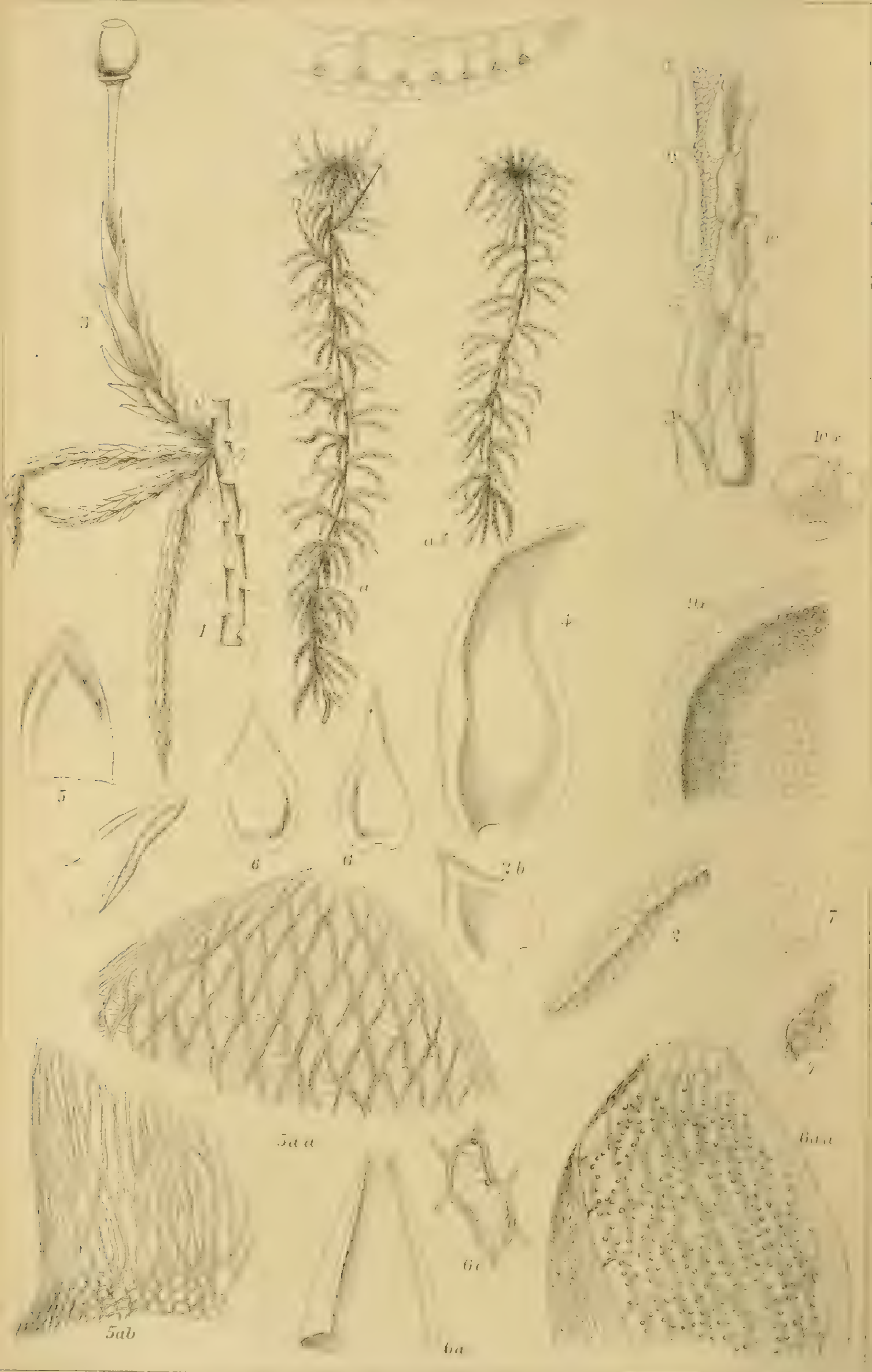
Donations to the Library and Cabinet, from March 6th to April 3rd, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*The Society.*
Journal of the Quekett Club, No. 18	*The Club.*
Journal of the London Institution, No. 13	*The Institution.*
Two Slides of Battledore Scales	*Dr. J. Anthony.*
Twelve Slides of Podura Scales	*S. J. McIntire.*

The following gentlemen were elected Fellows of the Society:—

B. D. Jackson, Esq.
Francis Fowke, Esq.

WALTER W. REEVES,
Assist.-Secretary.



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THE

MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1872.

I.—*On an Improved Reflex Illuminator for the Highest Powers of the Microscope.* By F. H. Wenham.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.)

Some retrospect may be needed to explain my reappearance before you on a subject which by this time may perhaps be considered threadbare and exhausted, but the very singular effects of the improved arrangement will, I trust, exonerate me from all undue intrusion.

It is now sixteen years since I described in a paper read before this Society (March 26, 1856) a mode of illuminating objects under the highest powers of the microscope. As then explained, this was strictly an opaque illumination, intended only for such objects as were mounted in balsam or fluid, as the intermedium permitted the slide and cover to be treated as an entire refractive plate, the upper plane of which, or cover, could thus be used like a speculum by throwing light down again from it as a total reflecting surface on to the objects beneath.

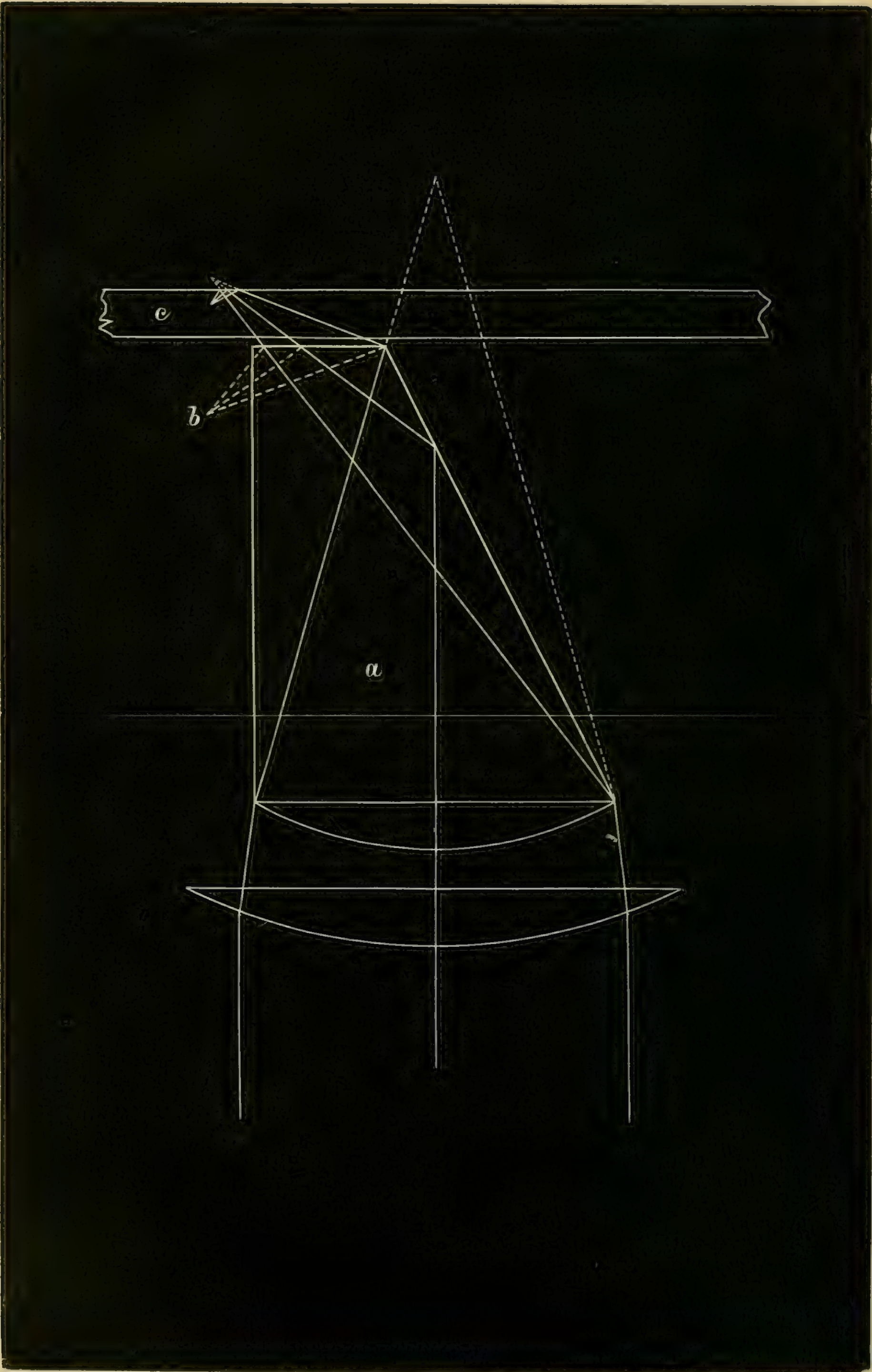
Several methods were figured and shown at the meeting for allowing rays above the angle of 41° to pass direct to the cover, through which of course they could not escape, but by the law of total reflexion would be thrown back again and intercepted by underlying particles or objects.

One plan was to patch on a right-angled prism by its long side with water, to the under surface of the slide, and then condense the light through either of the faces by lens adaptations. Another arrangement was by means of a solid glass parabola with a flat top, on which the slide was laid with water. This is now revived under the name of the " immersion paraboloid." The one that I then made I possess still, and employ occasionally. But the plan then preferred and in use now, without the slightest modification, is the combination of the ordinary parabola and the truncated lens, as this gave far more light than any of the others. The method has not been much used, as the number of objects in fluid or balsam capable of intercepting light as strictly opaque objects were remarkably

limited, and of course all structures that could not be seen in balsam, such as many butterfly scales, were quite excluded. I may remark also that immersion object-glasses (which, however, were not known or employed at that time) were not available, as the water between the cover and lens would prevent the reflexion, and the object could not be seen opaquely. I was not then aware of the possibility of viewing objects in a dark field with the largest apertures when mounted *dry*. This was an accidental discovery made a few years afterwards. Happening by mistake to take up my *Podura* slide and place it on the immersion parabola, I was surprised to perceive a few of the very numerous scales that I knew were there, shining out with great brilliancy, and found that these were the ones in contact with the slide itself; all those on the cover were invisible. It thus became evident that the effect was due to the interruption of total reflexion from the slide, the mere contact or presence of the object thereon attracting the light, or allowing it a passage through. That the object should under these circumstances collect light enough to be seen under high magnifying powers, could not have been anticipated; but so great is the intensity, that Col. Dr. Woodward has obtained some fine photographs of objects illumined this way. Except to friends at hand, I gave no special description of this method. The first notification of it appeared in one of my discussional communications to the 'Monthly Microscopical Journal' for July 1, 1869. Being usually either too indolent or indifferent to make public demonstrations in order to disseminate my notions, I merely publish them and leave them to be appreciated by the public or not according to their merits. Perceiving that many of our most eminent observers now use the truncated lens and parabola for this curious principle of illumination, I have exerted myself to improve upon the original plan, now sixteen years old, and which I must confess is in many respects a most clumsy and unhandy combination, and so difficult to manipulate, that experienced microscopists sometimes fail to show satisfactory results. In the first place it is difficult to patch on the truncated lens in the exact spot under the object you wish to observe. You have to shift it about several times before you can do this. After much trouble having got everything right for one object, you wish to have a look at a neighbouring one, you have to traverse the slide—lens and all—which will thus be set excentrically both in the parabola and relative to the object, and then to get a passable view you are compelled to effect an imperfect compromise by shifting either the mirror, the outstanding bull's-eye lens (which is always required for parallel rays), or by working the parabola up and down, and worst of all, if particles of dirt or *diatoms* are thickly strewn over the slide, they all become so brilliantly luminous, that the whole field is flooded with blue and white blurs, which greatly injure the definition by

impairing the sensitiveness of the eye. There is no possibility of *confining the light exclusively to the object to be viewed*, which is an important point. In this last respect my immersion paraboloid is not better, and also requires the bull's-eye for parallel rays.

The diagram, five times the size of the original (see p. 240), illustrates the plan that I have arranged to overcome these defects. *a* is a cylinder of glass half an inch long and four-tenths in diameter, the lower convex surface of which is polished to a radius of four-tenths. The top is flat and polished. Starting from the bottom edge, the cylinder is worked off to a polished face at an angle of 64°: close beneath the cylinder is set a plano-convex lens of $1\frac{1}{4}$ focus. Parallel rays sent through the lens, after leaving the lower convex surface of the cylinder, would be refracted to the point shown by the dotted lines if continued in solid glass, but by impinging on the inclined polished surface (which is far within the angle of total reflexion) they are thrown on the flat segmental top; here they would be totally reflected and beaten down again to the point, *b*, outside the cylinder; but if an object-slide, *c*, be laid over the flat top with an intervening film of water, the rays proceed on till the focal point reaches the upper surface or is slightly beyond it; here total reflexion now takes place; all the light is concentrated to a minute spot in the centre of the field of view of the microscope, and most of the rays are available for any object brought there by traversing the slides over the water top of the illuminator, which must be kept full without allowing any to run down the reflecting surface. It will be seen, in order to get the focal point in the centre of the microscope, that the lens centre must be excentric, but this does not involve the slightest inconvenience, as the excentricity only amounts to a little over two-tenths of an inch, and is so small that the same adjustment of the mirror serves during an entire revolution. The apparatus rotates on the focus as a centre. The management of this illuminator is very easy and simple; its fitting goes into the ordinary sub-stage, and has an independent rotary movement of its own, like that of Nachet's prism. The cylinder is brought up nearly level with the stage. The centre of rotation is set true by a dot on the fitting seen with a $1\frac{1}{2}$ object-glass. A drop of water is then placed on the top, upon which the slide is laid. The required objects *on the slide* are found by a low power, and may be distinguished by their brilliant appearance, while those on the cover are nearly invisible. The light is thrown up by either the plane or concave mirror. The former is generally the best and most controllable. The lamp should not be placed much beyond the stage, else its direct rays will get underneath and mar the blackness of the field. Having got the best effect, say on a *diatom*, or insect scale, by tilting the mirror, we now proceed to rotate the illuminator. During this the most exquisite unfolding of structure


c
b
a

takes place, opening out as it were into detail the form of bosses or ribbings. On that superb test, the *Podura*, for example, when the light is thrown from the apex to the quill, the whole scale is dotted over with bright blue spots laying in a zigzag direction; these are the most prominent parts or the club-end of the markings, which are nearest in contact with the glass. As the light continues to revolve, these gradually arrange themselves in the usual note of exclamation figures. In the reverse direction, or from quill to point, the blue spots are rather less distinct, and the object is most brilliant when the light is transverse to the scale. By making the light as oblique as possible in a direction from the point to the quill, I have for the first time been able to get a satisfactory view of the surface tissue of the *Podura;* it appears of a pale brown colour in waves or transverse undulations which reflect light visibly, and follow the zigzag of the markings. I need not, however, at present describe effects that will soon be known, further than to remark that *Amphipleura pellucida* assumed a substantial appearance, not seen in any other way, and at once displayed its striæ with an $\frac{1}{8}$th that had never resolved them before. As partly accounting for these effects, the black field has a material influence, and it has been long known that the resolution of very difficult striæ depends upon excessive obliquity of light. Now the most oblique that we can obtain by ordinary means, cannot strike the object at an angle greater than 41°, if it is either in balsam or on the slide, but on this principle we are dealing with rays entirely beyond this angle.

I will venture to anticipate a few objections that may be made to the arrangement. One might be that it is not achromatic. This would increase the expense, and I do not think will have the smallest advantage. The light spot is now so definite that its margin can be brought half across an *Angulatum* scale, at which there is a chromatic effect produced. This is by no means detrimental—quite the reverse, as some fine appearances are produced by it.

It may be said that the rays reflected from the lower end of the facet are just without the angle of total reflexion, and might enter true, and I had intended to stop off a small segment of the lens at this place, but found it so desirable, in many objects, to admit a little light, that I preferred it without alteration. It is easy to get a black field in all cases by mere mirror adjustment.

Also it might be objected that the light is always one-sided, which in many objects is well known to produce a host of false appearances; but this does not apply in this principle of illumination. It seems sufficient to get light into the object alone, whether sideways or not, so as to make it appear as a self-luminous body; and there is none of that coma thrown off like an overhanging veil, or

ghost object, long known as the "diffraction spectrum." I will, however, try another combination to act just opposite, and so make a double-barrelled affair of it, though I do not expect to gain much except in increase of light, and this is scarcely needed. Finally, it might be urged that we do not secure near the same average amount of obliquity of the rays as with the parabolic condenser. This is correct in appearance but not in effect, as there is a reason why the most oblique rays on this system of illumination are scarcely available. It is those nearest to 41° that are the most serviceable.

In my paper of 1856 I gave a diagram, showing that the use of water limited the angle to 160°. In order to secure the extra 20°, I recommended the use of oil of cassia or cloves for patching on the truncated lens; but the Rev. S. L. Brakey, who has made investigations on this principle of illumination, informed me that he found that it caused not the slightest difference whether water or a more refractive oil was used. Instead of waxing wroth at what seemed to demolish my pretty bit of theory, I accepted it as a fact to be reasoned upon, particularly as it allowed some latitude for improvements, and soon found that for very oblique rays the objects to appear luminous must be in absolute, or, indeed, in strong adhesive contact with the glass, otherwise these rays would not enter them; and as we now deal so considerably in the principle of total internal reflexion in microscopes and their appliances, I will finally submit a few remarks on the theory for further consideration.

When a ray of light falls on an internal surface of crown glass at an angle of near 41°, it can no longer be refracted outwards; its passage coming parallel with the plane surface, the whole is reflected back again at an angle equal to that of the incident ray. For monochromatic light, the limiting line between total reflexion, and transmission, or refraction, is extremely definite, but for ordinary light not so, as the coloured rays are refracted and pass in the order of their refrangibility. At about 40° a colour bow is seen across the prism, caused by the reflexion of a portion of the successive rays of compound light, but even then the boundary is very narrow, and comprised within an incident angle of one degree five minutes, consequently in all total reflecting arrangements the incident angles of the rays must exceed 40°. It is a fact well known to all those conversant with optical apparatus, that total reflecting surfaces must be kept clean, like those for direct transmission; any particles of dust, &c., on the former will abstract a portion of light from internal reflexion. It is exactly on this principle that the method of illumination described in this paper is based, but it appears to involve some conditions which do not hitherto seem to have been thoroughly investigated. I must call attention to an error which has been handed down in most optical

works up to the present time, which state generally that when a ray of light proceeds from a denser to a rarer medium, such as glass to *air*, beyond angles given, total internal reflexion will occur. But the fact is, that *air* has nothing to do with the phenomenon; its refractive power is so very small that it can only be discerned by dealing with enormous masses, and has been accurately determined by the difference between the real and apparent position of celestial bodies seen through the whole stratum of our atmosphere. Any portion of the back of a total reflecting prism that has the air withdrawn, by being set on a vacuum tube, will not show any difference at the spot where the air is absent; and if the whole prism is placed under the receiver of an air-pump, total reflexion will continue precisely as before: and as also bearing on the question before us, I may add that "Newton's rings," or the coloured bands seen between contiguous surfaces of glass are not caused by films of air, they are the mere effect of interval upon the colour undulations of light, considered upon the wave theory. When all air is withdrawn the colours are still there. I allude to this as not being altogether foreign to the present question, and to show that in what are deemed total reflecting surfaces there is some influence on light extending beyond that surface, and that external objects placed thereon *do not require to be in absolute contact* in order to abstract light. For very oblique incidences they must be much nearer, as I have found that the rays within a few degrees of 90° will not touch objects such as *diatoms*, but pass beneath them. At 41° the objects may be seen when they lie quite loose on the surface, and hence the reason why the least oblique rays are the most available. Sir Isaac Newton leaves it to be inferred that his colour rings are caused by air, and also that the same element performs its part in total reflexion. After he ends the experiments in the first part of his Optics, fourth edition, so accurately conducted and clearly expressed, towards the end in Book III. (which consists mainly of queries) there is the following remarkable passage in question 29, page 346, relating to total reflexion, which I will venture to quote:—"The rays of light in going out of glass into a *vacuum* are bent towards the glass, and if they fall too obliquely on the *vacuum*, they are bent backwards into the glass and totally reflected; and the reflexion cannot be ascribed to the resistance of an absolute *vacuum*, but must be caused by the power of the glass attracting the rays at their going out of it into the *vacuum* and bringing them back, for if the further surface of the glass be moistened with water, or clear oil, or liquid and clear honey, the rays which would otherwise be reflected, will go into the water, oil, or honey, and therefore are not reflected *before* they arrive at the farther surface of the glass, and begin to go out of it. If they go out of it into the water, oil, or honey, they go on because the attraction of the glass

is almost balanced and rendered ineffectual by the contrary attraction of the liquor; but if they go out of it into a *vacuum*, which has no attraction to balance that of the glass, the attraction of the glass either bends and refracts them, or brings them back and reflects them."

In this sentence, the word "*vacuum*" is in italics, as if the author doubted the influence of air, and both here, and subsequently, speaks more definitely of bodies in close proximity to the reflecting surface "attracting" the ray of light.

I must ask to be excused for this episode, on the plea of a desire to consider and discuss any obscure point in the principle under present attention.

Addendum.

Wishing to ascertain the distance at which objects on a total reflecting surface were rendered visible by light transmitted in consequence of their presence or contact thereon, I yesterday tried the simple expedient of Newton, and pressed a lens of long radius, on the back of a right-angled prism, in order to measure the distance by the colour rings. These were distinctly shown at all degrees below the angle of total reflexion; but as soon as the angle exceeded 40°, and the bow that marks the boundary had passed, the colour rings vanished, and the so-termed "black spot" alone remained, showing with peculiar distinctness. This is strictly a transparent circle through which all light is admitted and none reflected.

Under all incidences within the angle of total reflexion no colour appeared round the margin of the spot, except a very faint band seen by polarized light; but this was entirely due to the strain and compression of the glass, and had nothing to do with light transmission. The black spot was at its maximum size just after it had passed the boundary into the field of total reflexion; then, as the obliquity of the light was increased, the diameter of the spot diminished and became of less intensity, till at an incidence of about 5° it was almost invisible.

From this it may be inferred that no colour effects or decomposition of light can be produced in abstracting it from a total reflecting surface, for the reason that the object withdrawing that light must be brought into such close contact as to be within the distance of any undulation that can produce colour. This would be less than the ten millionth part of an inch; any distance beyond this will not affect the total reflecting surface. Now this distance is so small, that the question may be raised whether even the minutest atoms in the form of *diatoms* can lay sufficiently close, on account of their irregularities, to abstract the light, and whether their visibility, and also that of insect scales, must not depend

entirely upon adhesive contact caused by moisture or an oily exudation from the scale? If so, the effect would be enhanced by previously pouring over the slide a thin solution of Canada balsam in ether, so as to leave a mere trace, or sufficient only to ensure contact without the risk of spoiling the object by percolating its structure.

In English-mounted slides of *diatoms* and insect scales, some are always found detached from the cover and on the slide, and are shown well; but such is not the case with Möller's and others; they raise their covers off the slides by a ring of cement, and in many of them I have searched in vain for a solitary specimen that has become detached.

II.—*On a Silvered Prism for the Successive Polarization of Light.*

By J. W. Stephenson, F.R.A.S., Treasurer R.M.S., and Actuary to the Equitable Life Assurance Society.

(*Taken as read before the* Royal Microscopical Society, *May* 1, 1872.)

A paper on the "Successive Polarization of Light," by Sir Charles Wheatstone, was read before the Royal Society last year, wherein the author made known another means of producing successive polarization by the reflexion of plane polarized light from a plate of polished silver, and Mr. Spottiswoode delivered a lecture at the Royal Institution on these experiments, in which he stated that "if a ray of plane polarized light fall upon a metallic reflector, it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector, and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined at an angle of 45° to the plane of incidence, the two rays into which it is divided have nearly the same intensity. At an angle nearly 45°, which varies with the metal employed, but which is perfectly definite, the intensities become accurately equal. And further, if the angle of incidence have a particular value dependent upon the nature of the metal (for silver 72°), the retardation will amount to a quarter of a wave length. These two rays, on leaving the reflector, will re-combine, and in the last-mentioned circumstances become a circularly-polarized ray. Lastly, the direction of motion in this circular ray will depend upon the side on which the original plane of vibration is inclined to the plane of incidence; if, when it is inclined on one side, the circular ray becomes right-handed, then when it is inclined on the other, it becomes left-handed."

The purport of the present communication is to show how, by instrumental means, this method of producing successive polarization can be made applicable to the microscope.

The first essential condition of such an arrangement is that the ordinary polarizer should remain in its usual position beneath the stage of the microscope, and another is that the silver plate, from which the plane polarized ray is to be reflected, should have a flat and polished surface, and be, if possible, protected from oxidation.

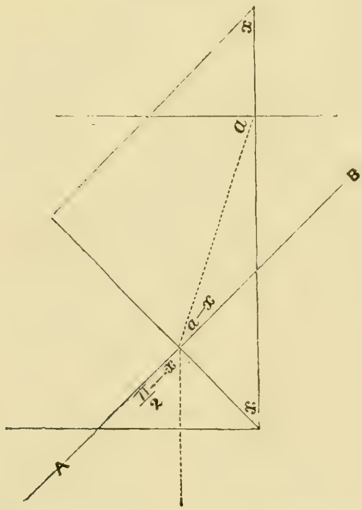
These conditions are fulfilled, by placing between the polarizer and the stage, a truncated glass prism, having two similar acute angles of such a magnitude that the plane polarized ray shall, on entering, be refracted from its course, and thus become incident on the hypothenusal side of the prism, previously silvered by the sugar of milk process, at an angle of 72°, whence after reflexion it will on emergence again be refracted, and resume the original straight line on which it was previously moving in the axis of the instrument.

It will be seen not only that the requisite angle of reflexion from the silver plate has been secured, but that the pure silver thrown down on the highly-polished glass presents a brilliant and even surface, which is effectually preserved from oxidation by the glass on which it is deposited.

The angles and dimensions of such a prism are easily determined if the index of refraction of the glass employed in its construction be given.

This will be readily perceived on inspection of the Figure No. 1, in which the dotted line represents a ray of parallel light, incident on the lower surface of a prism, to which the line A B is a normal. Let a be the circularly-polarizing angle of the metal employed, n, as usual, the index of refraction, and x the angle sought; then, as the angle of incidence will be equal to the complement of the angle

FIG. 1.



of the prism $\left(\frac{\pi}{2} - x\right)$, and the angle of refraction will be equal to the circularly-polarizing angle of the metal minus the angle of the prism $(a - x)$, we at once obtain the following relation :—

$$\cos. x = n \sin. (a - x).$$

From this equation by an easy transformation, which it is unnecessary to give, we obtain

$$\tan. x = \tan. a - \frac{1}{n} \sec. a.$$

As in the case which we have been considering the metal employed is silver, and the index of the glass used by Mr. Browning in

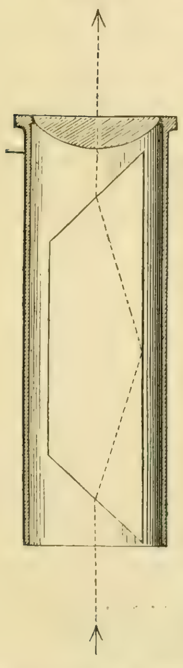
making the prism is 1·526, we find the value of x, representing the angles of the prism, to be $43\frac{3}{4}°$.

The formula being general, is applicable to glass of any density, or to any metal which can be thrown down on the glass, by simply substituting for a and n their respective values, but silver being more brilliant than any other metal, it is probable that, for microscopical purposes, no better can be employed.

Having thus determined the angles, we have now only to ascertain the *size* of the prism, by which I mean its length relative to its thickness. When the reflecting metal is silver, it depends exclusively on the value of n, and when this equals 1·526 the ratio of the thickness to the length of the prism will be as 1 to 4·122, and for all practical purposes this is sufficient; if greater accuracy is required, which is hardly possible, the new measure can readily be ascertained by the ratio of the sines to the sides of the triangles.

Generally speaking, a prism whose angles are $43\frac{3}{4}°$ should be about $1\frac{1}{2}$ inch long and three-eighths of an inch in both width and thickness; but this must of course depend in a great measure on the size of the Nicol which is used as the polarizer.

Fig. 2.



In the instrument before you, the prism—which is $2\frac{1}{16}$th long and $\frac{1}{2}$ an inch thick—is inserted in a brass tube, Fig. 2, on the upper end of which a small condensing lens of short focal length has been screwed for the purpose of condensing the light, and it being desirable at all times to know in what plane the reflecting surface is placed, a small point of german silver is made to project from the tube at right angles thereto; and still further, as a matter of convenience, for the same purpose, the outside of the tube is blackened for its whole length and half its diameter. Thus prepared, nothing is required but to place the tube containing the prism in what is called the selenite fitting of Beck's polarizer, by which it can be placed in any azimuth.

On the stage of the microscope is a film of selenite of uniform thickness, which has been divided in a line midway between the two neutral axes, the cut portion being brought together after inverting one of them, by which means a compound film has been formed, which exhibits simultaneously, as pointed out by Sir Charles Wheatstone, both right-handed and left-handed successive polarization—when the polarizer is turned, the tints of one portion ascend, while those of the other descend, and when it is at 45° and 135°, they

exhibit complementary colours—the film being that producing a green, the successive colours will be, on one side, G, B, P, V, R, O, Y, and on the other, in the reverse order.

If the selenite film be placed between the polarizer and the silvered prism it will be found, as explained in the paper previously mentioned, that on rotating the analyzing prism we shall have all the phenomena of successive or circular polarization, but, with the selenite between the analyzing plate or prism, when the polarizer is at 0 the polarized ray is reflected unaltered by the silver plate, "but when the polarizer is turned to 45°, 135°, 225°, or 315°, the plane of polarization of the ray falls 45° on one side of the plane of reflexion of the silver plate, and the ray is resolved into two others, polarized respectively in the plane of reflexion and the perpendicular plane, one of which is retarded on the other by a quarter of an undulation, and consequently gives rise to a circular ray;" when the polarizer is turned, "so as to place the plane of polarization in any intermediate position between those producing rectilinear and circular light, elliptical light is obtained."

To say more on the subject would be to detail the various experiments described in the 'Proceedings of the Royal Society,' in which Sir Charles Wheatstone's paper appears, and from which I have already quoted; but I may add that by this trifling addition to our instrumental means we are enabled to determine which is the thicker of two films of the same crystalline substance, to examine the coloured rings of crystals by light circularly or elliptically polarized, to produce the succession of colours with selenite or other plates as previously mentioned, as well as other phenomena which are "numerous and varied."

III.—*Structure of Battledore Scales.*

By J. Anthony, M.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.)

I venture to make some further observations on the structure of Battledore scales, inasmuch as, through the kindness of Mr. McIntire and Mr. Wonfor, I have been enabled to examine carefully a number of species of the Lycænidæ, to which these scales are peculiar; and more particularly do I bring this matter forward, because in making such examination I employed a mode of illumination which I found so very useful, that I would call the attention of the Society to it, as a valuable addition to our present means, for the investigation of structure under the microscope.

I will take this illumination first, as on it will partly hinge the correctness of what I have to describe with respect to the scales. In looking then at the various forms of these Battledore scales, it of course was desirable to make out clearly the structure on the surface by "reflected" light, in order to check the suspected fallacious appearances given under "transmitted" light. Now a ⅕th objective entailed such an amount of obliquity of light with respect to the plane of the object, that shadows, *quasi* rugosities, and possible extraneous matter on the surface of the scale became unduly prominent, so that very small bodies, such as the tubercular elevations I was seeking, were lost in all these exaggerations of light and shadow. Under these circumstances I bethought me of applying a device, often resorted to in practical astronomy, in the examination of close doubled stars, where the extreme contrast of brilliant light against a dark ground produces much confusion of vision, the said device for getting rid of this consisting in illuminating the field of the telescope artificially just to such an extent as to *reduce the violent contrast, without impairing the brightness of the image.* My adaptation of this principle to the microscope was accomplished in the following way: I illuminated the surface of the scale vividly from the side, by means of the usual plano-convex lens, and then, reflecting some light (by my mirror below the stage) through the condenser, I gradually brought that condensed light up to just so much brightness, as to very faintly illuminate the field and the object. The effect was all I could desire; the exaggerated shadows were done away with, the dominant illumination was still reflected from the object, and a careful balancing of these lights gave me the Battledore scales with a beauty, and the markings on their surfaces with a reality, which I had hardly ventured to expect. I suppose I may call this device "double illumination," and I take the employment of it for the microscope to be a novelty, as I have never heard of, or read of, its being so employed; however, remembering the Solomonian proverb that "There is nothing new under the sun," I dare say somebody will start up and say he has used this mode of

examination I do not know how many times! However that may be,—and I am sure I do not want to plagiarise—I can honestly advocate the use of this "double illumination," in the examination by "reflected" light of semi-diaphanous objects, which I take to comprise the majority of objects examined by the aid of the microscope.

To proceed to the Battledore scales. I am glad to say that I have found no reason to alter my opinion with respect to the tubercular bodies I described in a former paper as existing on the surface; they were plainly enough seen in *Ly. Alexis* by reflected light, but they could not be so readily made out in other species; this was unsatisfactory, as opposed to the usual harmony of the structure which is found to obtain in allied species; and moreover, as these *quasi* tubercles were more easily seen under transmitted light, it was suggested to me by an acute and intelligent observer, that these tubercles were really inside the scale, and formed part of a framework, and so held up, or pushed up the inner membrane of the scale in such a manner as to show on the surface as so many "lumps" or "bumps." I confess that in my inability to see the tubercles on the majority of the Battledores under reflected light, other than as shadowy, slight elevations, I was inclined to think that this "framework" view of the structure must be correct; but then the rows of distinct tubercles with stems and rounded heads on *Ly. Alexis* were so *very real*, and on a foreign specimen kindly sent to me by Mr. McIntire were absolutely so "staring" by reflected light, that I felt it necessary to re-examine more vigorously the whole series of scales, and to modify if possible that exaggeration of light and shadow which only dazzled and confused me. Hence the employment of the "double illumination" which I have described, and which made it at once evident that there was really no want of harmony of structure, inasmuch as I was now able to make out, so soon as the secondary light had removed the too great intensity of the shadows, that the tubercles were visible on *all* the species of Battledore scales, but that they varied in size in almost every one of the said species, being sometimes so small as easily to escape observation. I found the largest tubercles by far on the foreign specimen sent to me by Mr. McIntire, which scale I have sketched carefully.* The tubercles measure the $\frac{1}{6000}$th of an inch in length; in Alexis they are from $\frac{1}{12000}$th to $\frac{1}{10000}$th of an inch, while in many other species, as "Long-tailed Blue," I did not make them more than $\frac{1}{14000}$th of an inch.

Repeated observations have led me to recognize that the tubercles are in all cases placed upon a distinctly-beaded but narrow rib; the base into which the little column seems to fit,

* Dr. Anthony forwarded the sketch named, and the appearance is substantially the same as in the engraving illustrating his former paper. (See the number for January, 1872.)

looking broad and flattened—I use the term "fit," because not unfrequently I have seen the appearance, the tubercle displaced from the base to which it has apparently belonged, and lying on its side, a round hole in the flattened base strongly suggesting the original locality of the small peg-like tubercle. In one specimen, to which I can always refer by means of Maltwood's finder, a scale has received some crushing, and several of these little "pegs" lie about, looking as if they were knocked out of place; two of them appear reclining on the surface of the scale, and several more are seen on the glass by its side.

Although the Battledore scale is not strictly what would be called an object for the polariscope, there is no doubt parts of its structure, and particularly the tubercles and the ribs, do partially *de*-polarize the light, and therefore polarized light becomes valuable in examining the structure, and under a proper management of the analyzer and the sized pencil of light transmitted, the tubercles show with *extreme* brilliancy and reality, and moreover they seem to be placed as I have represented, in shallow pits in the surface of the scale in some specimens, while in others the ends of the pits seem semi-continuous, as if a row of tubercles stood in a groove, the sides of which were constricted at intervals. These pits may really be elevations, and due to "inversions of the image," but they do not look so, and I have put them down just as I saw them. I am so afraid of being thought guilty of exaggeration, that I have quite understated the effects which can be got in these tubercles by aid of polarized light. To employ this light to advantage, I prefer daylight, with tourmaline and Nicol's prism only, the latter behind the objective. For lamplight a pale-green ground from an appropriate selenite film seems an advantage.

A word in concluding, to those who may wish to repeat these observations on the markings of Battledore scales, or of kindred structures. Scales are best seen by "transmitted" *ordinary* light, when a "pin-hole" stop is placed like a small cap on the usual wide-angled condenser, and by being very particular that both flame of the lamp and object are in focus, or very nearly so, at the same time. When scales are looked at by "reflected" light, then I saw them at their best by bringing up a little transmitted light at the same time, such light being quite subordinate, and only for the purpose of rendering the black shadows transparent. A similar effect of course can be got by using a second lamp and "bull's-eye" at the other side of the microscope, or even in a minor degree by a bit of white card, placed in the stage beneath the object, so as to reflect light, but on the whole I prefer the first form of "double illumination" as equally satisfactory and far less troublesome.

April 27*th*, 1872.

IV.—*Beale's Nerve Researches.*

Dr. Beale in Reply to Dr. Klein.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.)

With reference to the criticisms of Dr. Klein, published in the April number of the Journal, I beg to offer the following remarks:—

1. The network described by Remak was not the same sort of network that was to be seen in my specimens.

2. Neither Schaafhausen nor Remak have given figures like those of the specimens published by me in the 'Phil. Trans.'

3. The network described by Kölliker was different from that seen and described by me. See, for example, my figure of the mucous membrane of the epiglottis, fig. 70, p. 168, 'Die Structur der Einfachen Gewebe,' translated by Victor Carus, Leipzig, 1862.

4. It is not possible to decide as to priority in such matters unless the figures given are compared. To say that So-and-so "described" a network or plexus before somebody else, goes for very little unless the exact sort of network or plexus is exhibited in drawings and placed before the observer. Neither ought the mere "assertions" of one observer to be brought forward against the observations of another.

5. My observations of the distribution of nerves to the bladder of the frog were made before those of Klebs, and perfectly independently of him.

6. That intermuscular networks are not "accepted" does not prove that intermuscular networks do not exist. I can show finer "intermuscular" fibres than the authorities cited by Klein. It is curious that the specimens of "intramuscular" nerves do not "keep," while mine of "intermuscular" nerves have been preserved for years.

7. I have, contrary to the statement of Dr. Klein, examined isolated muscular fibres in the frog, chameleon, and many lizards and snakes, as well as in many mammalia.

8. As regards the distribution of fine nerve fibres amongst epithelial cells, I have often tried to demonstrate them, but so far have failed to do so. I do not say that there are no nerves in the cuticle and in the conjunctival epithelium, but only that I have not yet seen any specimens that demonstrate them to my satisfaction. It is remarkable that those who believe in the distribution of nerves amongst epithelial cells do not agree whether they terminate in ends or form networks. While of all the observers who admit inter-epithelial nerve fibres, it is strange that not one has attempted to explain how they got there.

9. Dr. Klein fancies he convicts me of shifting my ground with regard to the constitution of the finest nerve fibres seen by me. Speaking of me, he says, "He admits (December, 1871) that his delicate fibres are compound, that they are fibrillar." He will find in my paper * that it is distinctly stated that "*each cord* (of the network)

* 'Phil. Trans.,' 1862.

is compound, and composed of numerous fibres which never anastomose." Again, in my Croonian Lecture, 1865, "Every fibre of this network is *compound;* so that perhaps the term 'plexus' more truly describes the arrangement."* My "previously-expressed opinions," therefore, do not clash with my "new views," as Dr. Klein wishes his readers to believe. Not only have I very distinctly stated that the fine nerve fibres I described were composed of still finer fibres, but I have given many figures of fine nerve fibres in many situations showing that they were so, and have published a diagram to show what I believe to be the arrangement, see figs. 399 and 400, 'How to Work with the Microscope,' third edition. Neither do I "abandon" my "former views," though I readily admit that "it is quite possible that there may be still finer fibres than any that I have been able to demonstrate." Dr. Klein's criticisms would be dissipated at once by reference to many drawings published by me during the last ten years. But Dr. Klein brings forward a quotation from p. 332 of the fourth edition, of 'How to Work with the Microscope,' and then remarks that I now admit that the nerve fibres are "compound," that they are "fibrillar," &c. Instead of commenting upon this, I will refer your readers to the very same page from which Dr. Klein quotes, and to the previous page, and to the plates illustrating my remarks, and ask them if Dr. Klein is justified in asserting that I have withdrawn from my previously-expressed opinions.

10. Dr. Klein seems to consider that the "nucleus" belongs to the "sheath," and asserts that the fibrillæ which come off from a non-medullated nerve fibre, either singly or in bundles, have absolutely nothing to do with any nucleus whatever. This may be true as applied to his own specimens, but it is absolutely incorrect as regards mine. There are nuclei to be demonstrated in hosts of fine nerve ramifications which come off from non-medullated fibres, and in every tissue in which nerves ramify.

11. There are other most positive assertions of Dr. Klein's which might also be refuted by specimens. Dr. Klein says that he has ascertained on one of my own preparations of the cornea that my view "by no means agrees with the facts"! Thus it would appear that in the course of less than a minute he had been able to discover what I had failed to make out after examining the same specimen for hours at different times during the last seven or eight years. However, one may feel content that one is not held up as a person perfectly blind, or as a melancholy example of perverted visual sense.

12. In England original observers in this department labour under great disadvantages. We are very few in number, and our work excites comparatively little interest. We have to condense our remarks into the smallest possible space, because English readers are provoked by long and exhaustive dissertations. I have myself always endeavoured to write briefly on anatomical questions, but to render my drawings as careful and accurate as possible, and at the same time to preserve the specimens from which the drawings have been

* 'Proceedings R. S.,' 1865, p. 237.

taken. If, therefore, a hostile critic merely refers to my description, he places me at a disadvantage, for no fair comparison can be made between contemporary anatomical labours unless the drawings are placed side by side. Again, an English minute anatomist, if he live long, may undoubtedly succeed in convincing his countrymen that he has really demonstrated some new facts; but the predisposition of many writers in England is against English anatomical work, and so strongly in favour of German observations, that the latter are sure not only of finding a prominent place in many of our journals, but of being favourably received and criticized. Observations made at home are often passed over.

It would be difficult to find a question of greater interest in its bearing upon the nature of important physiological and pathological changes than the distribution of nerves to capillary vessels. I am indebted to Dr. Klein for drawing the attention of his readers to the fact that these nerve fibres were first described and figured by me. My observations were made nearly ten years ago, and I believe Dr. Hughlings Jackson and Dr. Klein are the only writers who have yet drawn attention to the subject, which is so very closely connected with general pathology that it is hardly possible to discuss the nature of the nutritive changes in disease without taking into consideration the arrangement and action of the nerves distributed to capillary vessels. I hope that Dr. Klein's researches will render this and kindred inquiries more popular in this country, and that they will ere long receive that consideration from physiologists and physicians which they unquestionably deserve.

In conclusion, I may be permitted to remark that a scientific observer ought to consider himself fortunate if his researches have been allowed to pass for a few years without hostile criticism, and still more fortunate if, at the conclusion of a decade, observations made by him at its commencement have not been altogether forgotten and ignored; for few among anatomists have been allowed to work on for several years without having their "errors" very freely exposed and rendered too "gross" by a little.

LIONEL S. BEALE.

April 9th, 1872.

V.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Part IV.

Plate XIX.

Group B. Subsecunda. — Plants soft, laxly tufted, commonly growing with other species. Branches terete, their leaves erecto-patent or somewhat imbricated, usually subsecund, broadly ovate, truncate at apex, canaliculate-concave, the edges involute in the upper half, broadly margined. Dioicous.

2. Sph. tenellum Ehrhart.

Herbar. Petropol. (1795) teste Lindberg.

Plate XIX.

Syn.—*Sph. tenellum* Persoon MS. Bridel, Mantissa p. 1 (1819). Bryol. Univ. I, p. 4 (1826). Lindb. Torfm. No. 13 (1862). Hartm. Skand. Fl. ed. 9, II. p. 83 (1864).—Non *Sph. tenellum* of Bryol. Germ. nor of Funck, Taschenb.—*Sph. cymbifolium β tenellum* Bridel, Musc. rec. II, p. 24 (1798).—*Sph. obtusifolium β tenellum* Weber & Mohr, Bot. Tasch. p. 72 (1807).—*Sph. molluscum* Bruch, Regensb. Fl. 1825, p. 633. Bridel, Bryol. Univ. I, p. 753 (1826). C. Müller, Synop. Musc. I, p. 95 (1849). Wilson, Bry. Brit. p. 19, T. LX (1855). Hartm. Skand. Fl. ed. 6, p. 435 (1854). Schimper, Torf. p. 71, T. 21 (1858). Synop. p. 682 (1860). Berkeley, Handbook, p. 306, Pl. 2, fig. 3 (1863).

Dioicous. Plants *delicate, extremely fragile*, 2–6 inches high, but attaining a foot or more when floating, *densely clustered in soft tufts of a pale greenish yellow colour.* Stem simple or bipartite, straw coloured, covered with a *double non-porose cortical layer.* Ramuli abbreviated, single, binate or ternate, either all patulous or 1–2 deflexed, pale red, lax-leaved, not pointed; cortical cells very unequal, smallest quadrate, *largest flask-shaped, numerous, with the apex recurved and projecting, perforated, yellowish at the mouth.* Cauline leaves erecto-patent, reflexed, rather large, ovate-oblong, slightly narrowed toward the apex, margin incurved, with a broadish border, hyaline cells densely fibrose, sparingly porose, but toward the base free from both threads and pores.

Ramuline leaves patent or laxly incumbent, sometimes secund,

EXPLANATION OF PLATE XIX.

Sphagnum tenellum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b.* Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of base.
6.—Leaves from middle of a divergent branch. 6 *x.*—Transverse section. 6 *p.*—Point of same. 6 *a a.*—Areolation of upper part. 6 *c.*—Single cell from middle × 200.
7.—Intermediate leaves from base of a divergent branch.
9 ×.—Part of section of stem. 10.—Branch denuded of leaves. 10 ×.—Section of ditto.

broadly ovate and elongato-lanceolate, three toothed at apex, *incurved at edges in the upper half*, margined; hyaline cells strongly reticulato-fibrose, on the inner side of the leaf very prominent, confluent and perforated with pores, on the back separated from each other by the interposed chlorophyll cells, not perforated. Male plants in distinct tufts, rarely intermixed with the female, but resembling them; amentula small, orange-coloured. Fruit from the capitulum or from the side of the stem, on a more or less elongated pseudopodium; peduncular leaves large, imbricated, outer oblongo-lanceolate, inner ligulate, densely fibrose from the middle to the apex; capsule small, thin, ochreous-brown; spores large, sulphur-coloured.

Var. β *fluitans* Schimper, Torf. p. 72, T. XII. fig. 6. Very long and slender; pendent branches none, all the leaves distant, pseudopodia very long, with distant leaves.

Var. γ *longifolium* Lindberg M.S. Resembling the typical form; branches more attenuated at points, with longer leaves; cauline leaves with the hyaline cells all fibrose.

Hab. Spongy places on heaths, and wet hollows in hilly places. Not common. Fr. May. Occurs throughout Europe, but most frequent in the Scandinavian regions. In Britain it has been found in Lancashire, York, Sussex, Kent, &c., also in Scotland and Ireland. The specimens figured were collected on Keston Common, in Kent, by my friend Mr. Reeves. Var. β is stated by Schimper to have been found by Lesquereux at Marais les Ponts in the Swiss Jura. Var. γ was collected by Prof. Lindberg at Helsingfors, Finland.

A word as to the alteration of name. In Phœnogamic Botany, Entomology, and other departments of Natural History, the adoption of the first name by which a species has been described (dating from the establishment of the binomial nomenclature by Linnæus) is considered imperative; yet the synonymy of mosses is wofully confused, for Hedwig and others gave a new specific name as often as they changed the genus, a rule not sanctioned by the best authorities. Others may object with greater reason that the brief descriptions of the older authors are not sufficient to identify the species with certainty, yet it must be remembered that the actual specimens of very many of them are in existence, and their examination by a competent authority in most cases settles the question. Prof. Lindberg, who has worked so indefatigably at this unattractive department of botanical literature, has shown in his 'Rev. Crit. Ic. Musc. Fl. Dan.,' that this species is in the St. Petersburg Herbarium named *tenellum* in Ehrhart's own handwriting; this, however, without description, might not be allowed to stand, but the same species received the same name from Persoon, as proved by a specimen from him, preserved in Swartz's herbarium, and a description is given by Bridel in his 'Mantissa Musc.' (1819), the

leaves indeed being described as recurved at the point, which might perhaps refer to them in a dry state. Bridel also admits *S. molluscum* into the Bryol. Univ., but he only copied the description of Bruch (1825) without having seen a specimen. I have no hesitation therefore in adopting the name first given to the species.

VI.—*Crystallization of Metals by Electricity.*

By Philip Braham.

Plate XX.

The accompanying illustration represents the result of a series of experiments, and shows their most distinctive appearance as seen with a 2-inch objective.

Gold and copper are nodular in their formation, copper having the larger and more definite spherules.

Silver is peculiarly fine, and leafy, and has its main line of crystallization distinctly marked.

Tin may be considered the most beautiful object; its invariably rectangular crystal, straight direction, and rectangular deviation, are exceedingly characteristic.

Lead may be likened to silver enlarged; but the breadth and taper of the leaves, and its want of centre line, are very distinctive.

Zinc grows feathery, with a crystalline, fungoid appearance.

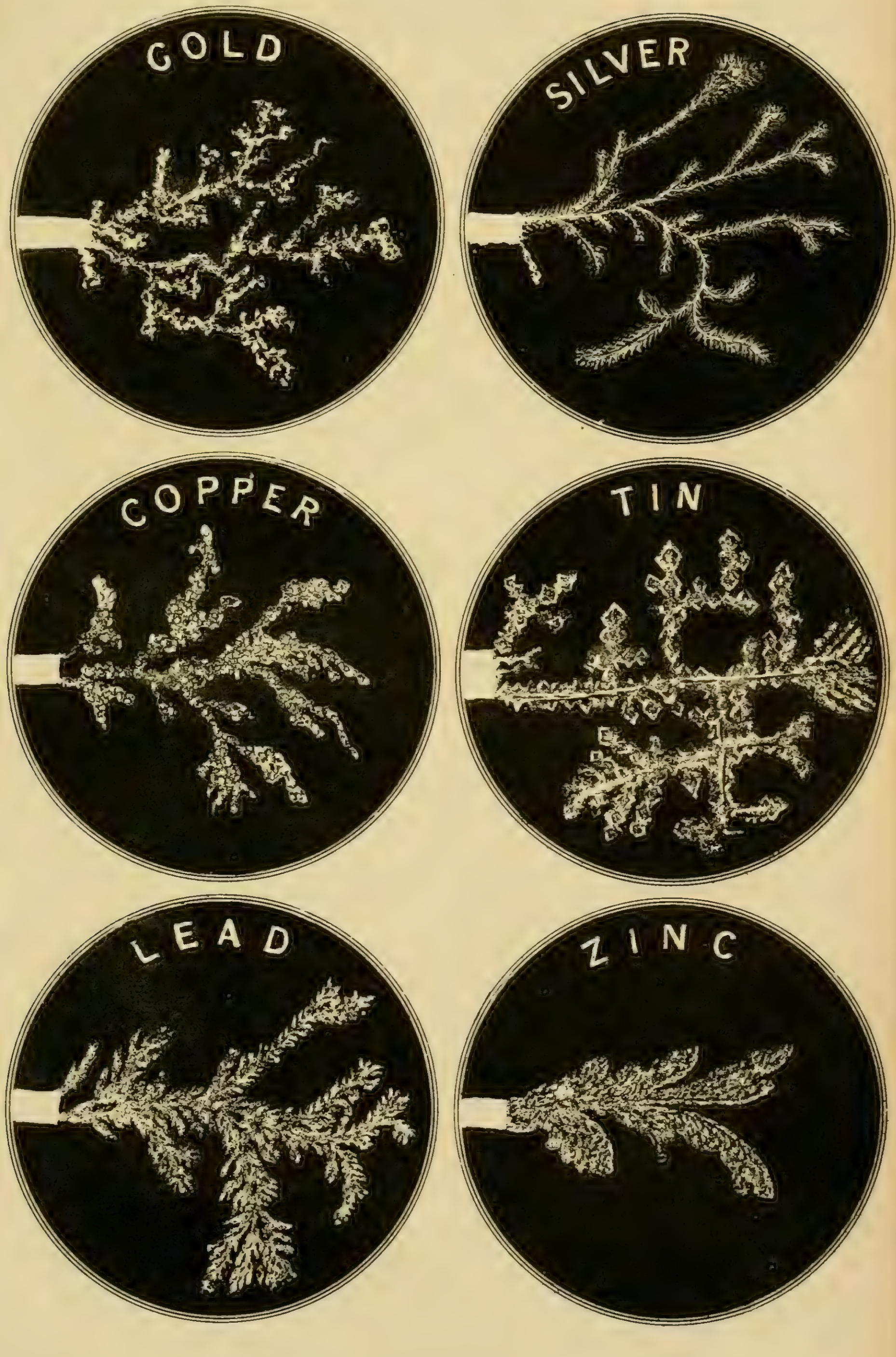
There are apparently other forms, which make their appearance with the same metals; but on careful investigation they are found to be only modifications of those illustrated.

It is worthy of notice that the two yellow-coloured metals have decidedly spherical formation, and that gold leaf transmits a blue light, and the salts of copper are beautifully blue. The size of the crystals depends on the regulation of the battery; the more rapidly they are formed the smaller they are.

I have tried the experiments away from the microscope, and have been generally disappointed with the result; the difficulty of manipulating away from it may be easily conceived by considering the solution to be a resistance to the electricity, and as the metallic crystals form that resistance is diminished, and a rush of crystals spoils the experiments.

I have succeeded in obtaining crystalline appearances from platinum, palladium, iron, bismuth, and antimony, but not of a very definite character.

The delicacy of some of the crystals will not allow of their being kept, and the beauty of their appearance is lost by drying. The more oxidable metals tarnish immediately, so that no ade-



Metals crystallized by electricity

West & Co. imp.

quate idea of the beauty of the experiments can be gleaned from mounted specimens, or sketches; they must be seen in the act of growth.

The formation of metals in their ores is strikingly similar, and there is every reason to believe their existence in the metallic state is due to the action of electricity, but whether thermal or magnetic is an investigation that could be profitably pursued; it would be of value if the direction of metallic veins could be determined as to their poles, and their position with respect to the surrounding minerals.

VII.—*On the means of distinguishing the Fibres of New Zealand Flax from those of Manilla or Sizal, by the Microscope.*

By Captain Hutton.

To an experienced eye, the difference between Manilla fibre and New Zealand flax, when in any quantity, is so great that I do not suppose that any mistake would occur in distinguishing between them; but when a small portion only is available for examination, as when Manilla rope is sparingly adulterated with *Phormium*, the case is different, and unless the easily-recognized marginal or mid-ribs of *Phormium* are present, the microscope will have to be resorted to before a satisfactory determination of the nature of the fibre can be made. Sizal, again, is still more difficult to distinguish by the unassisted eye from *Phormium*, than the latter is from Manilla, and I therefore propose to give the characteristics of each fibre, when examined with a microscope:—

In *Manilla*, the fibrous bundles are oval, nearly opaque, and surrounded by a considerable quantity of dried-up cellular tissue, composed of rectangular cells. The bundles are smooth, very few partly-detached ultimate fibres are seen, and no spiral tissue.

In *Sizal*, the fibrous bundles are also oval, and surrounded with cellular tissue. They are also smooth, and very few ultimate fibres project from the bundles. They are, however, more translucent than Manilla, and can always be recognized by the large quantity of spiral fibres mixed up in the bundles.

In *Machine-dressed Phormium* the bundles are translucent, and irregularly covered with tissue, being in places quite free from it. Spiral fibres can also be detected among the bundles, but not in the same quantity that is seen in Sizal. Many more ultimate fibres stick out from the bundles, which are flat instead of oval. In those places where the bundles are entirely freed from tissue, they are generally divided longitudinally into two or more smaller bundles, or *fasciculi*, and in these places the number of half-

detached ultimate fibres is greatly increased; these are, however, rarely broken, most of them having the end perfect. Spiral fibres are here absent.

In *Maori-prepared Phormium* the bundles are almost entirely free from tissue, and quite so from spiral fibres. They are always broken up into many fasciculi, which average, perhaps, some twelve or fifteen ultimate fibres in each fasciculus. Many ultimate fibres are semi-detached, and they are much more broken than in machine-prepared fibre.

If an examination of the fibrous bundles fails to give a satisfactory result, resource must be had to the size of the ultimate fibres which compose the bundles, and this will always give a certain proof of the nature of the fibre under examination, provided enough measurements are made to strike an average, and this can always be done, as one fibrous bundle a couple of inches long will yield an ample number of ultimate fibres.

In order to accomplish this, the fibre must be first boiled for two or three hours in a weak solution of potash, by which means it will be decomposed. It will then be found possible to isolate individual ultimate fibres from the rest of the bundle by careful dissection with needles under a microscope, the decomposed bundle being placed on a glass slide, in a drop of water. When thus prepared, it will be found that the ultimate fibres of Sizal will separate easily, those of *Phormium* with more difficulty; while it will require great care to prevent breaking those of Manilla when endeavouring to detach them. In fact, Manilla requires four or five hours' boiling in a tolerably strong solution of potash, before the ultimate fibres can be detached readily.

The following Table gives the average dimensions of the different ultimate fibres, made from a considerable number of measurements of each kind:—

	Length of ultimate Fibres in inches.			Diameter of ultimate Fibres in inches.			Thickness of Cell Wall in inches.
	Max.	Min.	Mean.	Max.	Min.	Mean.	
Sizal	·25	·20	·21	·00140	·00098	·00112	·00028
Manilla	·25	·17	·21	·00098	·00060	·00083	·00024
Phormium	·80	·13	·39	·00070	·00035	·00045	·00015

It will be thus seen that the average length of the ultimate fibre of *Phormium* is nearly twice that of either of the others, while the average diameter is not much more than half that of Manilla, which again is much less than Sizal. The cell wall also of *Phormium* is also much thinner than that of either of the other two.

NEW BOOKS, WITH SHORT NOTICES.

The Lens: a Quarterly Journal of Microscopy and the Allied Natural Sciences, with the Transactions of the State Microscopical Society of Illinois. Edited by S. A. Briggs. Chicago, U.S.A. 1872. —Our readers were made aware of the existence of this journal through several quotations which we made from it in a late number. We have now, in a few words, the pleasure of introducing it to them as a valuable and carefully-edited report, not only of the Society whose proceedings it is made the medium of communicating to the scientific world, but of the whole trans-Atlantic progress which is being made in the various branches of science connected with the microscope. Its first number promises well, for a moment's glance shows that its editor understands his business, and does not mean to allow any one department to get a too prominent position at the expense of any other. We therefore find that he has taken care to have every subject well represented, and we must, in the first instance, acknowledge the compliment he has paid us of selecting as his sole illustrated article, a paper which originally appeared in these pages, from the pen of Mr. M. Johnson, M.R.C.S. But, besides this, there are several able articles, more especially the "Conspectus of the Families and Genera of the Diatomaceæ," by Professor H. L. Smith; "The Preparation of Soft Tissues," by Dr. Danforth; "Microscopical Memoranda," by Col. Dr. Woodward; and "A New Method of Illuminating Opaque Objects under High Powers," by Dr. H. A. Johnson. Altogether, we are well pleased with the new Chicago venture, and we heartily wish it every success it so well and so thoroughly merits.

De la Microcythémie, par MM. Vanlair et Masius, Professeurs à l'Université de Liége. Bruxelles. Henri Manceaux. 1871.—We call attention to this work, not because we have any faith in the conclusion at which the authors have arrived, but because the theory they urge is a most important one if proved, and in order that those who have given attention to the subject may consider this theory. The writers are men very well qualified to make observations in microscopy and to write upon them; but we fancy in the present instance they are entirely mistaken, and that therefore their view is absolutely a false one. They give the term *microcyte* to a blood corpuscle which appears to differ somewhat from the ordinary ones, and they term the condition one of *mycrocythemia* whenever the blood presents a considerable number of those *microcytes*. We fancy the authors have rushed rather hastily to a hypothesis, for we can assure them, from our own experience, that there is hardly a human being in existence who does not present less or more of the characters they associate with *mycrocythemia*. At least, the writer's own observations, and they have been considerable, have led him to that conclusion. However, MM. Vanlair and Masius hold quite a different opinion, and they have expressed it very fully and supported it somewhat by

observation in the work under notice. The plate which accompanies the book is certainly, so far as we can see, opposed to the theory laid down in the volume. Still, our readers will, we trust, judge the book on its own merits.

PROGRESS OF MICROSCOPICAL SCIENCE.

Uniformity of Nomenclature in Objectives.—On this subject Dr. R. H. Ward has a paper in the 'American Naturalist' for March, which is of interest, and which we shall probably reproduce in a future number of this Journal. He thinks that the following questions still remain open:—Should the standard one-inch objective be characterized by magnifying ten diameters as used in the compound microscope, or should it be compared to a simple lens of actually measured focus or foci? Should the objective be named by its equivalent focal length, or by its amplifying power, or both? Should our standard distance of measurement be changed from ten inches [254 mm.] to nine and five-sixths inches [250 mm.]? From what point in the objective shall the distance to the scale be measured? At what point of screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power? Should the name *ocular* be substituted for "eye-piece" in general use?

Micro-photography.—Mr. Charles Stodder has a semi-popular paper on this subject in the 'Boston Journal of Chemistry.' It seems, says Mr. Stodder, that the history and advantages of microscopical photography are well given, though no reference is made to the corresponding disadvantages, such as the unequal applicability of the process to objects of different colours, and the necessity of representing a single focal plane or section of the object, while the different varieties of delineation by hand-work enable the artist, if sufficiently expert to know what he sees, and sufficiently candid to draw what he sees and not what he thinks he ought to see, to reconstruct to some extent the object, and represent at a single view the knowledge gained by many slight changes of focus. Unfortunately for their value as tests in this case, the so-called test-objects seem to be particularly suitable for photographic illustration. Of the Woodward photographs familiar to the writer, those of the test-objects are (probably necessarily) more faultless than those of the tissues, and are therefore tests of the corrections of the objectives and of the perfection of the illumination rather than of the general applicability of the photographic process. Of this latter question, but little understood as yet, the researches of Dr. Woodward and others give promise of an early solution.

The Structure of the Bloom or Wax of Plants.—Professor De Bary's paper on this subject in the Botanical Zeitung is thus abstracted

by a writer in the 'American Naturalist' (March). The original memoir contains thirty beautiful illustrations. The wax does not appear to be a simple coating over the surface, as though it might have been laid on liquid with a brush, forming a continuous layer. It is seen to be rather a dense forest of minute hairs of wax; each one sitting with one end upon the epidermis and the other either rising up straight or rolled and curled among its neighbours. This matting of waxen hairs often becomes so dense that when examined from the surface it presents to the microscope the appearance of a continuous layer, while a carefully-made section of the leaf, or skin of the fruit, shows its true structure. The question from what part of the epidermis or sub-epidermal tissue does the wax come, is most beautifully and clearly answered. Professor De Bary says that in the cell-contents there cannot be discovered the slightest trace of wax, and the statement that the chlorophyll is partly made of wax is totally erroneous. The locality in which it can be first detected is the cuticle and the cuticularized elements of the epidermis cells.

The Odontoblasts of Teeth.—Mr. T. C. White, the secretary to the Quekett Club, lately read before that Society a very interesting paper on the above subject. Mr. White agrees most generally with the views already laid down, and he showed the Society his method of examining the teeth, which is certainly of importance. His remarks on the subject of the odontoblasts are of interest. He says that about the seventh month of fœtal life the ossification of the tooth commences, and the dentine is represented by a cup-shaped scale capping the crown, and ultimately extending down the sides and embracing the whole of the upper surface of the pulp. It is at this period of their growth that the odontoblasts are most active, for they have the development of the dentine before them, and deriving a plentiful supply of nutrition from the plexus of blood-vessels beneath them, dentine is formed through their agency from without inwards, till the pulp being reduced to the size at which we generally see it by the gradual formation of the dentine, the odontoblasts become dormant, but capable of awaking to activity under the influence of certain circumstances of irritation; thus if caries attacks a tooth at a particular spot the tubuli in the dentine, through the fibrillæ in them, become consolidated at an equal distance from the point of attack all round it, and a barrier seems to be thus thrown up against the inroads of the advancing enemy; but unless such a remedial measure as the careful excavation of the carious portion of the tooth and subsequent plugging of the cavity be adopted, barrier after barrier may be thrown up but to be overcome. Even then the odontoblasts of the pulp resist by forming new dentine in its very substance, and it is only when inflammation and suppuration destroy the odontoblasts that this reparative process is annihilated. Mr. White's mode of preparing the tooth should also be read.—*Vide Quekett Club Journal*, April.

The Penis of the Flea.—Mr. W. H. Furlonge has a paper in the last number of the 'Journal of the Quekett Club,' in which he enters very fully on the anatomy of the flea. Most of his researches have

been done years before by a French zoologist; but there are some points of novelty in his paper. Particularly interesting is his description of the penis of this animal. Looking down, he says, upon the extremity of the extruded sheath, when expanded, a minute orifice may be observed through which the penis is projected. It is a dark-coloured, wire-like organ—presumably chitinous—which is capable of protrusion for about half the length of its sheath; but of its structure he is able to give no further description; in fact, he has only seen the extrusion of the organ in some four or five instances out of the dozens of male fleas he has had under observation, and on these occasions it was protruded and retracted with such rapidity that the eyes could hardly follow it. He has no doubt, however, that it constitutes one of the coiled rods or chitinous fibres, and that it is projected by the uncoiling, and withdrawn by the release of the spring-like coil. With regard to the copulation of this insect he says that when young and transparent specimens of the insect are selected this important process can be seen with remarkable clearness, and the animals being so closely locked together may be manipulated with great facility. Even when placed between the glasses of the compressor they will endure an amount of compression quite sufficient to render the abdomen of each insect perfectly transparent without the interruption of the copula. When thus examined the extremity of the sheath of the penis may be seen to be continually opening out and closing up, and by this action the spinous processes attached to the extremity of the sheath appear, as it were, to *grasp* the ovarian clusters which he has described; but he has not been able to observe the protrusion of the penis itself during the copula, nor at any time to distinguish spermatozoa in the female.

Description of certain Lepidodendroid Stems.—The following account of certain stems recently examined by Professor Williamson has been sent by him to the Royal Society, and is published in the 'Proceedings of the Royal Society,' No. 133. The examples vary from the very youngest, half-developed twigs, not more than $\frac{1}{12}$th of an inch in diameter, to arborescent stems having a circumference of from two to three feet. The youngest twigs are composed of ordinary parenchyma, and the imperfectly-developed leaves which clothe them externally have the same structure. In the interior of the twig there is a single bundle, consisting of a limited number of barred vessels. In the centre of the bundle there can always be detected a small amount of primitive cellular tissue, which is a rudimentary pith. As the twig expanded into a branch, this central pith enlarged by multiplication of its cells, and the vascular bundle in like manner increased in size through a corresponding increase in the number of its vessels. The latter structure thus became converted into the vascular cylinder, so common amongst Lepidodendroid plants, in transverse sections of which the vessels do not appear arranged in radiating series. Simultaneously with these changes the thick parenchymatous outer layer becomes differentiated. At first but two layers can be distinguished—a thin inner one, in which the cells have square ends, and are disposed in irregular vertical columns, and a thicker outer one consisting of

parenchyma, the same as the epidermal layer of the author's preceding memoir. In a short time a third layer was developed between these two. When the vascular cylinder had undergone a considerable increase in its size and in the number of its vessels, a new element made its appearance. An exogenous growth of vessels took place in a cambium layer, which invested the pre-existing vascular cylinder. The author distinguishes the latter as the vascular medullary cylinder, and the former as the ligneous zone. The newly-added vessels were arranged in radiating laminæ, separated from each other by small but very distinct medullary rays. At an earlier stage of growth traces of vascular bundles proceeding from the central cylinder to the leaves had been detected. These are now very clearly seen to leave the surface of the medullary vascular cylinder where it and the ligneous zone are in mutual contact; hence tangential sections of the former exhibit no traces of these bundles, but similar sections of the ligneous zone present them at regular intervals and in quincuncial order. Each bundle passes outwards through the ligneous zone, imbedded in a cellular mass, which corresponds, alike in its origin and in its direction, with the ordinary medullary rays, differing from them only in its larger dimensions. At this stage of growth the plant is obviously identical with the *Diploxylon* of Corda, with the *Anabathra* of Witham, and, so far as this internal axis is concerned, with the *Sigillaria elegans* of Brongniart.

The Modes of Origin of Infusoria.—One of the best and most highly suggestive papers that have for years appeared is that which Dr. Bastian, F.R.S., published in the 'Proceedings of the Royal Society' for April. It is so good that we envy the 'Proceedings' the possession of it. It is of considerable length, amply illustrated, and deals with the subject more simply and intelligently than we have seen in most of the writings on the subject. It certainly appears that the author is justified in the conclusions which he has drawn from a vast series of observations conducted during the past few years. This paper ends by stating that the phenomena which the author has described as taking place in the "proligerous pellicle" may be watched by all who are conversant with such methods of investigation. He does not require to call in the aid of the chemist; he need exercise no special precautions; the changes in the pellicle are of such a kind that they can be readily appreciated by any skilled microscopist. Just as he has supposed that living matter itself comes into being by virtue of combinations and rearrangements taking place amongst invisible colloidal molecules, so now does the study of the changes in the "pellicle" absolutely demonstrate the fact that the visible new-born units of living matter behave in the manner which he has attributed to the invisible colloidal molecules. The living units combine, they undergo molecular rearrangements, and the result of such a process of heterogenetic biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and rearrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially

similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the "properties" or "inherent tendencies" of the matter which displays them.

Where should the Characeæ be placed?—Mr. Frederick Currey, M.A., F.R.S., in delivering an admirable Presidential address to the West Kent Natural History Society, made the following observations on this important subject. He referred particularly to the researches made by Dr. De Bary upon the genus *Chara*, which have been published in the monthly reports of the Berlin Academy for 1871. The mode of reproduction in the *Characeæ* has been studied by one of the ablest of German botanists, Dr. Pringsheim, and in his opinion the cell upon which the fertilizing action of the spermatozoa operates is not the cell which immediately produces the pro-embryo, but a cell preceding that one by several generations. If this were so, the formation of the fruit in *Chara* would be similar to what takes place in the mosses. De Bary asserts that Pringsheim's view is incorrect, and that the ovum-cell, or oogonium, is not impregnated until towards the close of its growth, and that the phenomena of impregnation in *Chara* are not the same as those which characterize the mosses, but are similar to the reproductive phenomena which occur in *Vaucheria*. The supposed identity in the development of the fruit of the mosses and the *Characeæ* caused the latter to be arranged systematically at the commencement of the series of the *Muscineæ*, or as a special group in the division of moss-like plants. De Bary, upon the assumption that this supposed identity has been disproved, maintains that the only peculiarity common to the mosses and the *Characeæ* and not met with in other groups, is the form of the spermatozoa, and that this is not of systematic value. He considers that the *Characeæ* ought not to be remitted to the Algæ, amongst which they were formerly placed, because that group, in the sense in which it embraced the *Characeæ*, cannot be considered as subsisting, it being impossible to arrange the latter in any of the well-defined groups of Algæ. The *Characeæ* ought (De Bary says) to be ranked as a special group, not as a link of transition from the mosses to the Algæ, but as an independent group near the mosses on the one side, and the *Florideæ* and *Fucaceæ* on the other, allied also to some of the oosporous *Confervæ*, such as *Vaucheria*.

The Embryological Development of Limulus.—In addition to the work done on this subject by Mr. Packard, we understand that Professor Van Beneden, the eminent Belgian embryologist, on the other hand, has published a paper in the 'Comptes Rendus de la Société Entomologique de Belge,' in which, from a study of the embryological development of Limulus, he arrives at the following conclusions:—1. That the Limuli are not Crustaceans, as none of the characteristic

phases of the development of Crustacea can be distinguished; and that, on the other hand, their development shows the closest resemblance to that of the Scorpions and other Arachnida. 2. That the affinity between the Limuli and Trilobites cannot be doubted; and that the analogy between them is the greater in proportion as we examine them at a less advanced period of their development. 3. That the Trilobites, as well as the Eurypteridæ and Pœcilopoda must be separated from the class Crustacea, and must form, with the Arachnida, a distinct division.

NOTES AND MEMORANDA.

A Medical Man's Pocket-Case for Specimens, of an ingenious and simple description, is that described by Dr. I. N. Durnforth in the first number of the 'Lens.' It consists of a thick slab of plate glass, in which six deep cells are excavated. The cover consists of another plate of glass, of the same size, and the two are fastened together by a strong rubber band. By means of this simple device specimens of pus, of expectoration, of mucus, or of solid tissues, may be easily transferred without being either dried or soiled; and if a drachm vial of carmine solution be also carried along, and a drop or two be added when the specimen is first obtained, it will be stained ready for the examination by the time the physician reaches home. Wrappings of any kind of cloth or paper are totally unfit for the transportation of specimens, as every microscopist very well knows. The compound cell, which he has described, furnishes a cheap and simple, as well as compact and portable, means of carrying as many specimens as any physician would be likely to accumulate in a single day.

A Modification of Dr. Matthews' Turn-table has been suggested by the originator. Dr. Matthews said, at a late meeting of the Quekett Club, that most of the members would, no doubt, recollect that he had produced a self-centring turn-table, nearly two years ago (May 27th, 1870), and exhibited it at one of the meetings. It was then pronounced to be excellent, and it remained excellent for new slides, but in most cabinets there occurred a necessity for revarnishing old slides, and cells on these were not always found to be central. In such cases this turn-table would only correctly centre them, and thereby show their eccentricity, and its accuracy thus became a defect, although it was a defect consequent upon its perfection. He had, however, now devised a remedy for this by dividing the top of the table into two portions, so arranged that by sliding the upper part upon the surface of the lower, any required degree of eccentricity could be attained. This was accomplished very easily and simply, and he thought that the arrangement rendered the turn-table as perfect as could be desired; certainly he did not himself see what more could be done to it. One of the improved turn-tables was then

exhibited to the meeting, and its utility shown by centring a slide which had been eccentrically mounted for the purpose.—*Quekett Club Journal*, April, 1872.

An Improved Mode of Observing Capillary Circulation.—Mr. H. L. Smith says that if we grasp a frog in the hand and plunge it in water about as warm as can be conveniently borne, say about 120°, though he has never measured this, judging simply from the apparent warmth to the hand, we shall find that, in a few moments, the frog will become perfectly rigid; it may now be removed and laid upon a plate for dissection. Carefully opening and stretching the parts by pulling upon the fore limbs gently, or even cutting the bones if necessary, the heart may be displayed, showing the contraction and expansion beautifully; and if now the animal is placed in warm water, the lungs will immediately float out, and by a suitably contrived stage the circulation may be examined. "It is better, however, not to do this, but to draw out gently the large intestine by means of blunt forceps, and then spreading the mesentery on the glass of the frog-plate (I find it convenient to use a large one with an elevated glass, instead of one in the same plane, on which to spread the mesentery) we can observe the capillary circulation very nicely with a $\frac{1}{4}$th or $\frac{1}{5}$th inch objective, by dropping a bit of thin glass over the placo or with a higher power 'immersion.' Of course the parts opened must be kept moist and covered with a cloth, and a few drops of tepid water added from time to time. If the experiment has been properly conducted, the animal will remain perfectly quiet and the circulation will continue for hours; I cannot say how long, for I have never known it to cease until long after I had finished all the exhibition I have ever had occasion to make. If the frog is a large one, the mesentery can be spread out so as to afford the most magnificent exhibition of capillary circulation, with a distinctness, and under an amplification which will excite the greatest admiration and astonishment in anyone who has only seen it hitherto in the web, or the tongue. The objectives of a high power ought to be more tapering at the end than our American makers usually furnish them. In this respect some of the foreign objectives are superior. It would be very little more trouble to make the higher powers at the object end but little larger than the front lens, and thus infinitely more convenient for work than with the large flat surface which most of them now present. In fact, with a $\frac{1}{8}$th or $\frac{1}{12}$th American objective, as ordinarily made, it would be impossible to approach sufficiently near to the mesentery to focus on the smaller capillaries without striking some of the larger blood-vessels. If nothing more could be done, the front set at least might be mounted in a little projecting tip or nose, and if those who are ordering objectives will insist upon this, I doubt not the opticians will do their part."

The New Erecting Arrangement.—A writer who signs himself C. S., writes as follows to the 'American Naturalist' (April):—In the January number of the 'Monthly Microscopical Journal,' Dr. Ward describes "a new erecting arrangement especially designed for use

with binocular microscopes." The arrangement proposed by Dr. Ward will undoubtedly work as he proposes, but *cui bono?* It is an axiom in microscopy, as well as in other pursuits, that the simplest means of accomplishing an end is the best. Dr. Ward's arrangement is complicated and troublesome, and unless all the lenses are well made and carefully centred, definition will be injured. Dr. Ward is correct in his observation that the "*erectors usually furnished* [italics are his] are not good, and the use, otherwise satisfactory, of a good objective as an erector has not yet afforded the advantage of binocular vision." The first clause is correct, because "the erectors usually furnished" reverse, counteract, or destroy all the corrections which the opticians have taken so much pains to introduce into the objectives. The second clause refers to binocular vision. This has been completely accomplished by Tolles' binocular eye-piece, which has been in use and before the public more than six years. Without any change from, or addition to, its regular construction or use, it gives an image *erect*, *binocular*, and *stereoscopic* with any objective, from a 4-inch to a $\frac{1}{15}$-inch; and of course it may be used for dissecting by transmitted or reflected light with any objective having "working distance" enough for manipulation—certainly with a $\frac{1}{5}$-inch. The only objection, if it is one, against the instrument for this use, is that the "power," the amplification, is necessarily higher than with other binoculars. But where a *very low* power is wanted, I believe a pair of spectacles set with magnifying periscopic lenses will prove to be better than any binocular dissecting microscope yet devised. But the objection to the "usual erector" for monocular instruments remains. This was remedied by Tolles years ago; so long ago that he has forgotten when. He made erectors that did not disturb any of the corrections of the objective, but preserved them, and gave as good effects as were obtained without an erector.

A Note on the above Remarks, which we suppose to be by Dr. Ward, and which is signed R. H. W., says that it can hardly be necessary to state that Tolles' binocular eye-piece, with which the writer has sometimes worked, was ignored in the paper referred to, simply because there was no occasion to mention it,—it being no novelty, but an article whose properties have been perfectly familiar to American (and foreign) microscopists for years. It is only fair to add that the new arrangement, which can be added to any microscope at a cost of two or three dollars, has been used for months by several microscopists, who consider it extremely simple and convenient. That an erector (or anything else), however perfect, can be added to the objective and ocular, and give "as good" optical effects as would be obtained without the additional refracting and dispersing surfaces, is much disputed, and surely cannot be considered a conceded point at the present time. If Mr. Tolles is prepared to supply the market with erectors radically superior to those generally used, microscopists will doubtless learn the fact when it is announced, as he, R. H. W., does not find it now, in the catalogue of the Boston Optical Works or in their advertisements in the 'American Naturalist' and other journals.

Death of M. Alphonse de Brebisson.—It is with much regret that we have to announce the death of the distinguished naturalist, M. Louis-Alphonse de Brebisson, which took place on the 26th of April last. M. de Brebisson was a most distinguished naturalist. Though his absolutely microscopic work was small in amount, his additions to natural history were both numerous and important. He had reached his seventy-fourth year.

Death of Mr. Hodgson, F.R.M.S.—We regret to have to record the death, on the 4th of the month (May), of one of the oldest Fellows of the R.M.S.—Richard Hodgson, Esq., F.R.A.S., &c., of Chingford, Essex.

CORRESPONDENCE.

Sir David Brewster, Dr. Royston-Pigott, and the Photographic Camera Obscura.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—When I read in your Journal for the month of February a statement made by Dr. Royston-Pigott, that an "optical principle" had been laid down by Sir David Brewster involving as it seemed to me heretical doctrines, my curiosity was excited, and, as you are aware, I troubled you with a few lines upon the subject.

Now that I am, by the favour of Dr. Pigott's reply, put in possession of the words of Sir David, I must confess to a considerable amount of surprise, and I will add, of regret. As Dr. Pigott is responsible for the revival of what was generally regarded as a defunct controversy, I will, with your permission, address these remarks more particularly to him.

The doctrine to which I object is, that "Lenses of four inches aperture and upwards, when used upon a minute object, yield nothing but a mere jumble of hideous images, proceeding from upwards of 200 different points of the compass," &c.

I object equally to the doctrine that, to get rid of the "distortion in photographic images caused by the stereoscopic effect of such a lens, it is necessary, or proper, to reduce its aperture to that of the pupil of the eye."

With regard to the aberrations ascribed to lenses of four inches aperture and upwards, it may be observed that whatever aberrations may be created in any lens by rays of light diverging 150° as in the microscope, it does not follow that where the rays are parallel, or virtually parallel, as in the camera obscura (in which the divergency never exceeds a single degree), the same aberrations will take place.

In support of that view I may appeal to the object-glasses at the Greenwich Observatory, in which no aberrations of the kind exist;

and which are supplied to the most minute objects with perfect success.

It would be unbecoming in me to question any of the optical opinions of Sir David Brewster, but I think I may venture to break a spear with Dr. Pigott, even under the ægis of so eminent an authority.

It will be admitted, I presume, that the arguments of Sir David Brewster, so far as they relate to photography (of which alone I am treating), rest entirely upon the analogy between the camera obscura and the human eye; and I agree that there could be no better foundation upon which to proceed; but in reasoning out the parallel between the two, Sir David, by a most unaccountable oversight, has entirely overlooked the principal factor in the question, namely, the *length* of the ocular focus.

If the ocular focus be assumed as $0 \cdot 8$ of an inch, represented by a; the aperture of the pupil $0 \cdot 2$ inch, represented by b; the focus of the camera obscura 12 inches, represented by c; then it is evident that, in the human eye, the aperture of the pupil is one-fourth of the ocular focus; and the proportionate aperture in the camera lens would be $\frac{12}{4} = 3$ inches, not $\frac{2}{10}$ of an inch, as assumed by mistake.

Or, substituting the real or any other measurements, and adopting the symbolic but accurate and comprehensive language of Dr. Pigott, the proportionate aperture would always be $\frac{b\,c}{a}$.

I do not suggest that a trifling oversight of this nature will in the slightest degree affect the scientific character of Sir D. Brewster; but it vitiates the whole reasoning of which it is the foundation, and throws some light upon the justice and propriety of certain "*unsparing* criticisms."

Finally, I feel entitled to call upon Dr. Pigott, either to point out the error in my calculation, or to accept it as true. In either case, I apologize beforehand for any expression I may have used which may have given him pain.

Your very obedient,

G. S. CUNDELL.

P.S. If not encroaching too much upon your space, now that I have cleared my stomach of controversy, I should like to say a few words in extenuation of the faults of certain unskilful practitioners in photographic portraiture, who have brought so much obloquy upon their whole fraternity: I must, however, inform Dr. Pigott that I have nothing to do with that beautiful and now very important art.

Everything may be overdone, and certainly everyone must occasionally have seen in photographic portraits an undue prominence given to stereoscopic effect. I am, however, by no means in favour of abolishing it altogether, by the use of lenses of $\frac{2}{10}$ of an inch aperture.

Do we not, to our great advantage, see everything stereoscopically? Painters, I believe, paint all that they can see, and, as they use both

eyes, they must see, and paint, stereoscopic effects. Did not one of the most able and learned of them all announce the true stereoscopic theory 400 years ago, and practise its teachings, as all great painters have done ever since? Why, then, seek to exclude it from photographic portraiture?

In thus advocating a certain amount of stereoscopic effect, I would limit it to that quantity under which people see one another; that is, to the quantity due to a lens of $2\frac{1}{2}$ inches diameter, that being the average space between the pupils of our eyes, which I may be permitted to call their "stereoscopic aperture." With such a lens, a head would be reproduced as we see it in nature, without the hard, cutting, lines of monocular vision, which all painters deprecate, and avoid.

Dr. Pigott will excuse me passing over a variety of subjects introduced into his letter: I confine myself entirely to a small branch of the optics of photography.

G. S. C.

Object-glasses.

To the Editor of the 'Monthly Microscopical Journal.'

Padnal Hall, Chadwell, Essex, *March* 4, 1872.

Sir,—In No. XXXI. of this Journal, for July, 1871, page 36, there appeared a short notice from Mr. Tolles describing an experiment intended to prove that a pencil of rays of more than 82° can pass from a balsam-mounted object with a wet-front or immersion lens. This has nearly escaped my recollection, and perhaps is forgotten by most of your readers. In the succeeding Journal, while giving Mr. Tolles credit for truth and honesty in the measurement of the angle shown, I pointed out how the error had arisen from a refraction caused by the position of the under hemispherical lens. Knowing that it is no easy task to bring this within the comprehension of minds not familiar with optical science, I took the pains to show, by a direct experiment, that the angle was considerably diminished by water. Canada balsam might have been tried in the same way, but I objected to injure my object-glasses by immersing their fronts in a tank of balsam merely for the sake of demonstrating a result which I thought the most obtuse could foresee. In water a dry-lens aperture of 170° was reduced to 100°. Mr. Tolles seizes upon this to show that I am wrong by my own experiment on the point—that a greater aperture than 82° cannot pass from a balsam-mounted object. Had the experiment been repeated with the front immersed in a tank of balsam in lieu of water, it needs no demonstration to convince those conversant with the laws of refraction that the aperture would then have fallen to below 82°, and further, though the aperture is 100° on an object *in* the water, yet when that object is in balsam as usual, and under the water, that the rays would again be refracted outwards, reducing the angle from the object to within 82°. Mr. Tolles is unable to perceive this, and so let it remain. Mr. Tolles has accepted the only

condition under which the full aperture can be brought to bear on a balsam-mounted object, *viz.* that of the tiny hemispheres. I am glad of his announcement that he has succeeded in this, and should like to see the same thing done in this country, particularly with large-aperture glasses, say higher than $\frac{1}{8}$th. But to end the matter, I must positively decline any further reply to Mr. Tolles on the question of the passage of rays through refracting surfaces, as it is futile for him, and a waste of time for others. I have merely to refer to his Fig. 2, page 117 of the last Journal, as my apology for this. Starting from the focal point, and accepting his position that the refractive power of the covering glass, interposed medium (say balsam instead of water), and front lens are nearly the same, the rays may be taken as straight till they reach the outer convex surface : and here is a gross error permanently recorded in black and white, for at the angle at which they are made to reach that surface, instead of emerging in the direction he has shown, the refraction would be so small that they would proceed more nearly in a straight line, so that no possible back combination at present known could take them in, or in due course bring them to a focus behind.* The science of optics ranks amongst the most exact of all, and no fanciful direction of rays is admissible. In the object-glass question there have been imaginative diagrams *ad nauseam*, when all deductions should have been taken from drawings of the real thing. Mr. Tolles has made combinations that have done well in practice, and if he will give us the correct radii, diameters, distances, &c., of any one combination that has been actually constructed, and transmitting a full aperture in balsam, and show us the passage of the rays through to prove the result, then I will strike my colours, or if he will furnish the drawing, the rays shall be traced through for him. Nothing beyond what has been known before, has yet been learned by the correspondence, unless the fact that in America the *single front*, differing in thickness and slightly in radius, is now employed both for the dry and wet lens in their best object-glasses. I fairly claim to be the originator of this system. I made but one object-glass with a triple front (in the year 1850). I then found that the radii of the contact surfaces of this gave a large excess of over-correction or *negative* aberration for parallel rays, and under-correction or *positive*† aberration with divergent ones. This induced me to project the mean refractive rays through the entire combination drawn as a large diagram, which analyzed the cause, and led at once to the single front with a thickness proportioned to correct the aberrations. Some particulars of this are given in my paper "On the Construction of Object-glasses," commencing with this Journal. I am happy to find that this formula has now become almost universal, and that the triple front may be considered as

* Fig. 1 is still more erroneous. In the dotted-line lens of Mr. Tolles, shown within the copy of my front, it may be seen at a glance how his dotted ray will emerge nearly as a radius.

† These terms are so thoroughly known and descriptive of the conditions of the marginal rays both in chromatic and spherical corrections falling without or within the central ones, that I am not disposed to reverse them on any theoretical ground, or that they are "rule of thumb employed in the workshop."

obsolete. At the time the belief was slow, though in the few years following I had made a $\frac{1}{5}$th, two $\frac{1}{8}$ths, two $\frac{1}{12}$ths, one $\frac{1}{25}$th, and one $\frac{1}{50}$th, all on this system, and which were duly mentioned by the President of the Microscopical Society in his annual report at the time. I was always glad to challenge comparisons, in order to prove the superior advantage of this construction, yet it has only recently been adopted by some of our most eminent English makers; and however this may now, in some slightly-modified form, be announced as a "newly-discovered system," yet the principle remains the same, and like in all exact sciences there is but one plan of perfect construction.

I have reason to believe that I am now unfolding a rigid law in respect to the relative foci and correction of the front and back combinations, that applies equally to the $\frac{1}{2}$ inch and upwards in all adjusting glasses. It has the merit of simplicity to recommend it, and as soon as I can develop and define its conditions, I will give the data from which I anticipate that mathematicians can work with advantage to the science.

Yours very truly,

F. H. Wenham.

Dr. Pigott and Mr. Hennah.

To the Editor of the 'Monthly Microscopical Journal.'

Milton House, Clarence Street, Brighton, *April* 17, 1872.

Dear Sir,—I cannot allow myself to be condemned by default, as would be the case if Dr. Pigott's note, at page 173 of the last number of the Journal, remains uncontradicted.

Will you kindly afford me space in your next issue to state that the photographs alluded to by Dr. Pigott are the same as those presented to the Society in March, 1871. They were the result of *original* experiments, and so far were they from being taken from Dr. Pigott's rods, or in any way in connection with him (as he ingeniously suggests in the note), that *I* arranged the rods and took the photographs, and then sent them to Mr. Curteis, who, for convenience of display at the March meeting of the Society, further mounted them in a chromatrope frame before he placed them in Mr. Hogg's hand.

I never heard of Dr. Pigott or anyone else having experimented in the same way, until *after* my experiments had been published in London; and I can prove that Dr. Pigott, when he wrote his note, was fully aware that the photographs were from rods mounted by *me*.

I am sorry so far to occupy your space, but as the photographs and rods were submitted to the Royal Microscopical Society, as *original* illustrations in proof of the difficulty of determining structure—as well as a caution against too readily accepting Dr. Pigott's statements, I must, in justice to the Society, as well as to my own position, distinctly assert the originality of experiments undertaken to ascertain

the value of the beaded appearances on which was based an attack on our observations and objectives, which is still, in my opinion, most insufficiently supported.

I will not now do more than express a hope that Dr. Pigott's record of personal observations is more precise than his statement of facts.

I am, dear Sir, faithfully yours,

JNO. H. HENNAH.

MR. ROSS'S OBJECTIVES.

To the Editor of the 'Monthly Microscopical Journal.'

GROVE HOUSE, NEWBOLD STREET, LEAMINGTON, *April* 4, 1872.

DEAR SIR,—To the numerous readers of the Journal who, like myself, look out for its successive appearances with expectation and pleasure, perhaps this note may not be without its interest. I have no doubt we all feel a degree of justifiable pride in the name of Ross, as historically connected with the achromatic microscope, and regret the loss of father and son, who kept so well to the front in the constant march of improvement. I am happy to say there is every probability of the old House keeping up its well-earned reputation. A few days ago I received a box of objectives from the present firm, which were so superior that I feel it to be only an act of justice to call attention to them. The $\frac{1}{10}$ths surpassed any other glasses of like focus I ever received from them, and we all know how T. Ross prided himself on this combination: the beauty of their performance was equalled only by their *facile* resolution of tests ever considered difficult for this power. Another glass of remarkable character and excellence was a "new" $\frac{1}{5}$th, which acts most beautifully as a dry lens, and by manipulating the adjustment, *without change of front*, performs admirably as an immersion lens. This double performance as dry and wet lens is shared, as far as I know, only by Powell and Lealand's beautiful $\frac{1}{8}$th. This $\frac{1}{5}$th resolves the *Cymatopleura elliptica* of Möller's Probe Platte. I would here remark that Möller having used such a thick slide to mount the diatoms on, prevents the proper illumination requisite to bring out the best results. I found by gently warming the slide I could remove the cell containing the diatoms. This I did, and remounted it on very thin glass, with the best result, and without disturbing in the least the beauty or perfection of the test-plate. All Ross's glasses, I understand, are now worked from new formulæ, which promise a still further point of excellence: they have a new $\frac{1}{40}$th under construction, from which much is expected, and no doubt justly so.

I am yours truly,

THOMAS BIRT, M.D.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *May* 1, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

The Secretary called the attention of the meeting to a simple and inexpensive method brought forward by Mr. Richards, of adapting a second objective by its being screwed into a brass plate and placed on the stage, and centred by the stage movement after the plan proposed by Mr. R. H. Ward, M.A., for erecting the image when using the microscope for dissecting purposes. This plan was referred to in the January number of the Transactions.

A communication was read from Dr. Beale in reply to the criticisms of Dr. Klein upon his (Dr. Beale's) recent paper "On the Relation of Nerves to Pigment and other Cells or Elementary Parts."

Mr. Hogg said he thought Dr. Beale laboured under some misapprehension with regard to Dr. Klein's paper published in the Transactions. He believed Dr. Klein had no intention of claiming more for German physiologists than the discovery of the fine nerve fibre in connection with vessels and other structures contemporaneously with Dr. Beale, and it would appear therefore they had some claim to be regarded as discoverers in a field of research, which had gained not a little from Dr. Beale's investigations. Dr. Klein showed that Remak in 1843 first described "a terminal network of nerve fibres," and that investigations on nerve structure had been carried on from that time to the present by his countrymen. He also showed that the carmine process failed to bring out clearly the finer nerve fibres, while the chloride of gold process, now so much employed by the German school, demonstrated in a most satisfactory way the finer nerves of the epithelial layer of the cornea, as well as those permeating deeper seated structures; and, what was still more important, these finer nerves could be seen by the use of low powers. A power of 250 diameters would show even the varicosities of the minutest fibres. He would refer to Dr. Klein's mode of making his preparations, which he thought of some importance. He (Dr. Klein) first passed a fine silk thread through the cornea of the living frog, to set up a certain amount of inflammatory action, and as soon as possible after death the cornea was removed and steeped in the chloride of gold solution. In this way the solution permeated thoroughly every portion of the corneal structure. The light, subsequently acting upon the connective tissues, stained them a beautiful violet colour, while the nerves were left

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

quite black, and seemed to stand out in bold relief when examined with the binocular. He (Mr. Hogg) had had an opportunity of examining quietly Dr. Klein's specimens, and he must say they were very beautiful and wonderfully clear.

The President then read a paper "On the Difference between the Skull of the Thrush, as one of the highest classes of Birds, and the Skull of the Common Fowl."

Dr. Murie asked the President what was the difference between the structure of the bird's jaw, and that of the mammal, and especially that of the human being.

The President then described by means of a series of diagrams the mandibular arch, as it exists in the mammal, the bird, and the fish. He drew the attention of the Fellows to the unity of the plan of creation, exhibited by the metamorphoses which took place in the development of animal structures.

Mr. Slack said they could all highly appreciate the philosophy of such a paper as the President had just read, although but few might be able to follow the whole of the details. They could not help being very much struck by the way in which science has recently enlarged the ideas of men with regard to the unity of the plan of creation. He thought also that many would be disposed to see a striking confirmation of the truth of the views held by the Darwinian school. He begged to propose a vote of thanks to the President, which was carried unanimously.

The President, in acknowledging the vote, said he wished it to be distinctly understood that the whole of the work of which he had described the results was microscopic work. It was all the result of observation made through the microscope, although perhaps not with the highest powers.

The President asked Dr. Braithwaite whether he could not, as a botanist, confirm the value of the method he (the President) employed in his investigations.

Dr. Braithwaite replied he could most certainly do so. The botanist began with the lower forms of plants, and traced them up to the higher forms, carefully marking their affinities, and taking a proper view of the structure. As he ascended, he would notice that the cells of the lower plants underwent extraordinary metamorphoses, which, however much they might seem to indicate that the vessels found in the higher could not originate from simple cells; that they do so is nevertheless proved by the fact that in the embryos of the highest forms of plants nothing but cells are to be found.

Mr. Wenham then read a paper "On an Improved Reflex Illuminator for the Highest Powers of the Microscope."

Mr. Brooke said it seemed to him that Mr. Wenham had described a much more manageable plan of illuminating an object by rays obtained by total reflexion, than that which he had himself proposed many years ago, or even than that which Mr. Wenham had brought forward some time since. The plan in question was one by which reflected rays could be more readily brought to bear upon a minute object than anything else that had yet been introduced to the notice of the Society.

A vote of thanks was then given to Mr. Wenham.

A paper was read from Dr. Anthony, F.R.M.S., containing further observations on battledore scales of butterflies.

Mr. Wenham said some of the scales he had examined were remarkable for their thickness. They appeared to have an internal structure, which led him to conclude that there was something more than inflection which kept those scales in form. He should much like to know how they were situated. Sometimes they stood up like hairs on the surface of the scale.

Mr. McIntire said, in the scale which Dr. Anthony had figured, and which was then under the microscope, the little tubercular bodies were unmistakable. He was inclined to think that in these particular scales they lay between the membranes; but of their particular functions he could not possibly express any opinion. In one or two of the scales under notice, when pressure had caused the tubercles to lie on one side, the dome-shaped top of the tubercles and the base and the neck could be distinguished. On the other battledore scales he was certain they were there also, but whether inside or outside the membrane he could not be certain. In *Alexis* they seemed to be definite elevations on the inner surface of the scale (that is, on the membrane next the wing of the insect). He did not think any elevations were to be found on the exterior surface. He had never examined the battledore scales during the life of the insect, but had never experienced any difficulty in examining them *in situ* on a dried wing as opaque or transparent objects.

Mr. Slack said he had examined a great many of these battledore scales with which he had been supplied by Dr. Anthony and by Messrs. McIntire and Wonfor, and he was disposed to think that the view held by Mr. Wenham respecting their structure was substantially correct. He had tried the scales in all manner of ways, looking at them under glass, and again as uncovered objects, and under each condition they presented most vexatious difficulties. They seemed to him to be bags, and in dealing with them the ordinary modes of illumination utterly failed. With a great many objects that really presented elevations there was no difficulty in examining them by means of unilateral light, and ascertaining what the true structure was; but directly this mode of illumination was applied to these objects, their whole appearance became confused. If illumination was effected by a small central pencil of light, such a plan would doubtless facilitate getting certain effects, but the total absence of shadow left the observer in considerable doubt. It moreover placed him somewhat in the position of looking down upon a small object so as to produce much foreshortening. He had once tried some of the scales entirely uncovered, and he came to the conclusion that upon those were to be seen little bumps. If you focus down with the fine adjustment, upon these battledore scales the first thing seen was the outline of a bump on the upper surface of the scale, and he believed that on the membrane below was a similar bump. Mr. Wenham had noticed something like a columnar form, and if you imagined that these two membranes, the upper and lower, are separated by little hollow pillars, rounded

at the top and bottom, you got something like what Dr. Anthony figured, but inside instead of outside. Dr. Anthony says he had used polarized light. He (Mr. Slack) had tried polarized light, placing the polarizer under a $\frac{4}{10}$ condenser, and fixed the analyzer just over one of Powell and Lealand's immersion $\frac{1}{8}$ths. With that he got abundance of light. When the prisms were so arranged as to give a perfectly luminous field, the tops of the supposed pillars permitted the light to pass freely, and they looked white, with a little shading off at the edges. By turning the prisms so as to get a perfectly dark field, all the thick parts of the scale became brilliantly luminous. The margins or rims of the supposed columns were quite bright, and the centres dark. Hollow pillars covered with thin membranes might produce these effects. Among some peculiarities he had noticed in the most perfect scales series of ridges or ribs. One of the scales viewed as an uncovered object suggested the idea that the true form of the markings might be that the columnar pillar referred to was formed by bifurcations of the ribs, the ribs opening and the two halves uniting again so as to make hollow columns.

Mr. Hogg, referring to Mr. Wenham's remarks, said he thought Dr. Anthony looked upon these peculiar battledore scales as somewhat allied to glandular structures in animals. This species of butterfly was mostly found "on the banks where the wild thyme grows," and it was quite probable that they sucked the essential oil from the flowers of the plant, and stored it up in these scales, which occupied an intermediate position beneath the more brilliantly-coloured scales, and the membraneous structure of the wing. It was somewhat remarkable that these scales were only found on the wings of males. This would seem to favour the notion that the oily fluid stored up in them might in some way contribute to the greater brilliancy of the plumage of the males and cause them to be more attractive to their female companions. Mr. Hogg took exception to some of the terms employed by the various speakers, as those of "tubules," and "tubercular elevations," which did not convey the same meaning, although drawn exactly alike. Mr. Slack, he thought, was right in his representation of the structure in question, and in speaking of it as "a tuberculated membrane," that is, a raised part of the membraneous portion of the scale. Whereas, if Dr. Anthony's representations were correct, the structure consisted of a stalk or pedicle, terminated by a nodulated extremity projected out from the surface of the membrane. In such a case it could not be called *tuberculated* in the sense in which the term was usually employed.

Mr Stewart said he had lately examined some dry specimens of the battledore scales of *Polyommatus Alexis* and of other Lycænidæ. He quite agreed with Mr. Anthony as regarded the shape of the stud-like bodies, but thought they were situated between and not outside the membranes of the scale.

The battledore scales of *P. Alexis* were spoon-shaped, having the hollow of the bowl directed towards the membrane of the wing; they were inserted into the wing at an angle of 45°, close to the point of attachment, and underneath the larger scales.

In no position of the scale could he see the studs as upstanding processes, and in torn and also in uninjured scales which lay on their outer surface, the first thing seen on carefully focussing down was the thin and sometimes finely-beaded deeper membrane of the scale and the head of the stud; a slight alteration of focus would next bring into view the base of the stud, the coarse ribs, and the delicate superficial membrane. He consequently believed the studs to be buttresses or pillars, whose bases were attached to the superficial costate membrane, and whose rounded heads were fixed to the deeper membrane. If this interpretation be admitted as true, he thought that the structure in question might give strength to the scale. An analogous instance might be seen in the wing-case of many beetles (Dytiscus, &c.). In this case pillars connected the two membranes of the wing, apparently to give strength, and to afford a space in which the trachea, &c., might ramify; these pillars had been mistaken by some for modified crystals, but their real nature could readily be detected by examining a vertical section of the wing-case.

He fancied that the purpose served by the scales referred to might be to maintain at a favourable angle for catching the light the large iridescent scales; indeed, that they might be compared to the true tail feathers of the peacock, which are short, strong, and plainly coloured, being used to support the brightly-coloured feathers of the back, which constitute the false tail.

Donations to the Library and Cabinet from April 3rd to May 1st, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*The Society.*
Journal of the London Institution, No. 14	*The Institution.*
Popular Science Review, No. 43	*Editor.*
President's Address and Report of the West Kent Natural History Society for 1871	*Society.*
Der Vogelschutz. Von G. R. V. Frauenfeld	*Author.*
Die unseren Kulturpflanzen schädlichen Insekten. Von Gustav Künstler	*Ditto.*
Ueber die Weizenverwüsterin Chlorops tæniopus Meig und die Mittel zu ihrer Bekämpfung. Von Professor Dr. Max Nowicki	*Ditto.*
Die Grundlagen des Vogelschutzgesetzes. Von G. R. von Frauenfeld	*Ditto.*
Die Pflege der Jungen bei Thieren. Von G. R. von Frauenfeld	*Ditto.*
Verhandlungen der Kaiserlich-Königlichen Zoologisch-botanischen Gesellschaft in Wien, 1871	*Society.*
Bulletin de la Société Botanique de France, 1869–70 and 1871	*Ditto.*

Edward Harris, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

Brighton and Sussex Natural History Society.*

February 8th.—Ordinary Meeting. Mr. Hollis, President, in the chair.

A very pleasing diversion to the business of the evening took place in the shape of the presentation to Mr. J. Colbatch Onions, one of the Honorary Secretaries, of a handsome dining-room clock, with visible escapement, striking the hour and half-hour, furnished with perpetual calendar, and bearing the following inscription:—"Presented to J. Colbatch Onions, Esq., by the Members of the Brighton and Sussex Natural History Society, as a mark of their esteem and gratitude for his services."

Messrs. C. Carpenter, F. Smith, F. W. Salzmann, and J. Schweitzer, were elected ordinary members.

Mr. Wonfor announced the receipt, for the library, of the 'Proceedings of the Linnean Society,' 1871–72, and No. 54 of the Linnean Society's Journal, from Dr. Addison; 'Observations on the Climate of Brighton,' by Mr. F. E. Sawyer, from the Author, and the 'Fauna of Devon; 'Crustacea, Podothalmata,' the 'Parasitism of *Orobanche major*,' 'Experiments on Spontaneous Generation;' and the 'Boring of Mollusks, Annelids, and Sponges into Rocks, Wood, and Shells,' by Mr. E. Parfitt, from the Author.

Votes of thanks were given to the donors; to the Dispensary Committee, for the use of the Board-room for the soirée; and to Messrs. R. Glaisyer, Penley, G. Scott, C. P. Smith, Saunders, Walsh, and Wonfor, for their valuable services in arranging and carrying out the soirée.

Dr. Addison, F.R.S., then read a paper "On the Natural History of Cures and Healing."

The inquiry, "What is healing?" involved us in others. What is growth? especially as respects the first formation of blood-vessels in the embryo. What is inflammation? If dust entered the eye, inflammation arose; if a thorn pierced the finger, and, breaking, left a piece behind, there would be inflammation and suppuration; if a dead part had to be thrown off, the phenomena were inflammation, suppuration, and granulation. Granulations were characteristic of healing. They consisted of myriads of newly-formed blood-vessels, and the coats of the stomach were composed of globules or cells, not distinguishable from the colourless cells of blood. It was in a vast assemblage of such globules or cells, that new vascular tissue, new blood-vessels, were formed. Exactly the same materials and phenomena as might be seen at the earliest period of growth.

Some years ago an American gentleman exhibited a Hydro-incubator, in London, and allowed him, Dr. Addison, to purchase eggs at every period of incubation, and to examine them. On one occasion an egg was opened after seventy hours, when the parts were seen covered by innumerable embryonic vessels, which had nothing but globules in their structure. Without any other disturbance, a watch-glass was placed over the broken part of the shell, and the egg

* Report by Mr. T. W. Wonfor.

returned to the incubator. Twenty hours after, a great number of new and collateral vessels, strangely divergent from the natural ones, had been formed, to meet, as it were, the damage done by the premature breaking of the shell. All these vessels had nothing but globules in their composition, and were easily obliterated by moving the point of a needle in their midst.

Other facts of healing might be compared with embryo growth. If a tendon was ruptured, or a bone broken, great heat attended the first stage of reparation. In all parts the heat of the first stage of healing was above that of the neighbouring parts. These facts correspond with the heat necessary for the growth of the first blood-vessels in the egg. Blood-vessels of the embryo were destroyed by the gentlest handling, so likewise the new vessels in a healing sore, bled at the lightest touch.

In the embryo, colourless elements of blood constitute the first vessels; so, also, in the new vessels of healing parts. All parts of the embryo were more vascular than afterwards, when growth was further advanced: so with healing parts, granulations were, at first, more vascular than they were when turning into fibrous tissue. It took longer to repair a broken bone than it did to repair a ruptured tendon, which again was longer in healing than a wound in the skin, because in embryo growth, tendons were completed later than skin, and bones later than tendons.

It was well known that inflammation preceded granulation: what had it to do with healing? When fully-formed blood-vessels were called upon to contribute to healing, a change took place in their coats from elastic fibrous to fragile globular tissue, a kind of retrogression to the embryo state. The change might be said to do them violence, as it altered their state of cohesion and elasticity to one of fragility. Hence the pain of inflammation, demanding quiescence in parts preparing for healing. New blood-vessels for reparation could not possibly partake of the circulation without this change in the coats of the existing vessels. At the commencement of inflammation, blood has been seen depositing its colourless globules upon the interior of the vessels; these globules or cells, gradually accumulating, at length interpose between the stream of red blood and the elastic coats of the vessels, and substitute a fragile globular form or fibrous tissue. Vessels so altered were virtually embryonic, prepared to set off new vessels and new growths. Inflammation was the necessary preliminary to granulation. The two main constituents of the body were the solid parts and the blood. Injury to the latter was as fertile a source of inflammation as to the former. In extreme parts the antecedents of an injury could be seen, and the form, extent, and gravity of inflammatory action was measured by the triviality or gravity of the damage done. But in all that relates to blood-poisoning, the antecedents and the amount of damage done were hidden from view, but inferences might be drawn conversely. When small-pox was passing away through a moderate number of kindly pustules, and scarlet fever through a series of cuticular exfoliations, and in other fevers, when the crisis was moderate in type and duration, we might infer that no great damage

had been done to the blood, and that nature had found a good outlet for disordered and disarranged elements, the antecedents or cause of the fever.

When great mortality attended fevers or the dangerous ordeal of inflammatory abscess, suppuration and ulceration had to be passed through before recovery—much greater damage was implied, a greater augmentation of the fever poison, and a greater difficulty in eliminating it. There could not be a shadow of a doubt that blood was liable to as many accidents as the solid parts, and that inflammation was a struggle of nature to set matters to rights—a struggle which might be described as a more or less complete return of blood and blood-vessels within its area to the early stages of embryo growth. If an injury to a solid part was slight, no retrogression of blood-vessels took place. The blood rushed to the part, but there was nothing to be repaired; the injured parts recovered themselves, and there the matter stopped—this was simple inflammation. If more damage had been done, then the blood-vessels returned to the embryo state, and according to the gravity of the damage and the healing work to be done, so was the need of surgical help urgent. Likewise, in trivial cases of blood disorder, if the morbid humour could be worked out without material change in the coats of the vessels, the action was comprehended under the term rashes, pimples, &c.,—examples of simple inflammation. But in graver examples of blood-poisoning, when blood-vessels must retrograde to satisfy the needs of the damaged blood, the action was comprised under other terms, all referring to different degrees of the physiological work designed for the cure of local injuries, blood-poisoning—a physiological work amply vindicated its place in Natural History, without in the least degree diminishing the need of the experienced superintendence of the physician in regulating, controlling, and diverting the various forms in which it appeared in blood disorders.

Reading Microscopical Society.*

March 6th, 1872.—Captain Lang presided.

Mr. Tatem contributed a short paper "On a Presumed Form of Actinophryan Life." In it he referred to swarms of germs which eventually assume a spinose form, and have a forward movement effected by means of a vibrating filament. After some time these settle down, flatten themselves, and, throwing out pseudopoda, become *Actinophrys sol.* In this stage they were said to be very voracious, eating largely of *Astasia margaratifera.* By subsequent change the pseudopoda disappear, and the earlier forms are resumed; but these the writer believed to be, in reality, advanced forms.

Captain Lang exhibited a slide of grouped diatoms presented to him by Mr. Cole. This contained forty-nine forms, systematically disposed, and it would be difficult to find a fault in their arrange-

* Report supplied by Mr. B. J. Austin.

ment, perfection as to individual valves, or in the general brightness and cleanliness of the mounts.

He also brought for the inspection of the members a large collection of slides, kindly lent by Captain Perry, of Liverpool, principally selected diatoms, many being excessively rare and valuable. The collection also included many Foraminifera, scientifically arranged, and a beautifully-grouped slide of spicules of *Hyalonema*.

Mr. Tatem exhibited mounts of ant lion (*Myrmelion formicarium*) and of the tick of red deer.

The Croydon Microscopical Club.

Soirée.

[We regret that this and other reports have stood over so long; but it was quite impossible to insert them earlier.—Ed. 'M. M. J.']

The second annual soirée under the auspices of the above Club, was held on Wednesday evening, November 8th, 1871, in the Public Hall, which presented the same gay and brilliant appearance as characterized their first soirée last year.

Appended is a list of the more noticeable objects sent for exhibition:—

The President (Mr. H. Lee), in addition to six microscopes, contributed two emeu's eggs, laid in the grounds of W. H. Peek, Esq., M.P., of Wimbledon; sections of a very fine ammonite, from the lias of Lyme Regis, Dorsetshire; and casts of trout, painted by H. L. Rolfe. His microscopic objects chiefly consisted of fossil wood in coal; fructification of marine algæ; crystals in colloid silica; stomach teeth of Nereis, a sea-worm; anatomy of star-fishes; and embryo crustacea.

Mr. Frank Buckland sent for exhibition a skin of a Polar Bear, recently shot by Captain Gray, in the Arctic regions. The preserved skin of the monster was 8 feet long, and more than covered the large table on which it was spread. There were also a nondescript monster, constructed by the Japanese of papier-maché and wood, with a photograph of a similarly artificial animal; and a bottle containing a rat, preserved in spirits, whose history was a somewhat singular one. It had pushed its head through a ham bone, and being unable to extricate it, it lived for many months with the somewhat novel collar around its neck. This formed the subject of two humorous contributions by Mr. Lee, to the columns of 'Land and Water,' in 1867, which are well worthy of being reprinted, but which, of course, have no rank in a purely Scientific Magazine.

The treasurer to the club—Mr. J. W. Flower—was unavoidably absent, but his son, Mr. J. Flower, exhibited a series of skulls of quadrumana, or the monkey tribe.

Dr. Carpenter's collection of flints found in Croydon, attracted a large share of attention. In some of these flints were to be found beautiful specimens of the echinus. Here were also fossils from the

Antwerp Crag, and others from Swanage, and imbedded in the latter were to be seen perfect specimens of willow leaves. Some extraordinary shells of the *Malleus vulgaris*, and a gigantic oyster-shell, from India, with two cases containing beautiful specimens of marine Polyzoa, neatly mounted by Mrs. Bury, of Thornton Heath, were greatly admired.

Mr. Whitling exhibited a microscope and a graphoscope, and with the latter some very beautiful views.

Dr. Adams contributed largely to the Natural History department by sending specimens of the Australian Diver, the native bear of Australia (Koali, or "biter"), the duck-billed Platypus, or water-mole, the Echidna, or porcupine ant-eater; calabash, from the West Indies, Australian war-clubs and spears, some of which had been lent by Mrs. Lockyer, who also sent some Chinese chop-sticks. A wag, on observing that no object had been placed in Dr. Adams's microscope, wrote on the card which bore his signature, "*Ex nihilo nihil fit*," which caused no little amusement, in which the Doctor participated when he discovered the joke that had been perpetrated in his absence.

The cases of butterflies which had been artistically arranged in imitation of a flower garden, by Mr. Cooper, gardener to Mr. Paget, were greatly admired, not only for the harmonious blending of colours, but also for the taste displayed in disposing of these handsome insects to the best advantage.

Mr. Thomas Cushing exhibited three microscopes with interesting objects, amongst which was a most beautiful collection of natural plants; twenty-four photographs of scientific instruments constructed for the great trigonometrical survey of India; and a differential polariscope, which fully merited the large share of attention it received.

The following members of the Croydon Club also exhibited microscopes:—Mr. H. Ashby; Mr. J. Berney; Mr. A. Crowley; Mr. P. Crowley; Mr. H. Noakes; Mr. T. Brindley; Mr. E. Sturge; Mr. F. C. Clark; Mr. Loy; Dr. Owens (circulation in *Vallisneria spiralis*, a very beautiful object); Mr. F. West, jun.; Mr. McKean, jun.; Mr. W. H. Snelling; Dr. Strong (sections of wood); Mr. Sigsworth (antennæ of cockroach, polarized); Mr. C. W. Hovenden (three microscopes and a collection of old and rare coins); Mr. Cooper; Mr. Linney (an illuminated copy of the ten commandments under the microscope); Mr. Perry (some quaint old Chinese etchings); and Mr. Spencer, who exhibited a crystallized lens of the eye of a cod-fish, with regard to which some curious calculations were made by Sir David Brewster on measuring the object. He found that the number of fibres in each spherical coat was 2500; the number of teeth in each fibre, 12,500; number of teeth in each spherical coat, 31,251,000; number of fibres in the whole lens, 5,000,000; and number of teeth in the whole lens, 62,500,000,000. Mr. H. Noakes displayed, under the microscope, some foraminiferous sand; Mr. J. S. Johnson, *Deutzia scabra* (polarized light), and a beautiful collection of small shells from Devonshire; Mr. H. Lee, jun., petal of geranium; and Mr. Manners, wing of *Ornithoptera Richmondii*.

Commencing at the centre table, the first thing that struck the

visitor was an extraordinary double-erecting binocular, which enabled two persons to examine the same object at once. This instrument, exhibited and invented by Mr. J. W. Stephenson (of the Council of Royal Microscopical Society), was considered to be quite a novelty and a speciality of the evening. Captain Tyler displayed sections of Japanese glass-rope sponge, and many other rare specimens of sponge, both silicious and calcareous. Dr. Millar had on view several beautiful objects; the Rev. T. Wiltshire and Mr. Hilton displayed admirably-mounted objects – spicules in flint, and spicules out of chalk. Mr. Burr contributed a series of photographs showing the different phases of the moon; Mr. Butler exhibited a beetle's eye, magnified 400 times, on every facet of which was to be seen the second-hand of a watch, and as each of these hands moved simultaneously, some speculation arose as to the cause of this somewhat singular phenomenon. It was courteously explained that the eye was transparent, and that the second-hand of a watch had thus been reflected upon each facet, which produced this amusing optical delusion.

Dr. Braithwaite, Vice-President of the South London Microscopical and Natural History Club, exhibited a palate of a water snail; Mr. Ackland, a group of young oysters, shown by the aid of a new side-light reflector; Mr. F. Hovenden, one of the Infusoria; Mr. Robinson, an admirable specimen of native gold; Mr. Cottrall, a five-pound Bank of England note, magnified to its normal size; and a collection of selected diatoms. Mr. How (inventor of a new and improved microscope lamp with porcelain shades) exhibited the pollen of the marrow; Mr. Rogers, abdomen of a foreign bee, which excited much attention; Mr. Jackson, two admirable mountings of a fungus on an elm leaf, and wing of a Brazilian butterfly; Mr Neighbour, a number of brilliant glass stereoscopic views, principally illustrative of Swiss scenery; pieces of mignonette and chrysanthemums, splendid objects.

Mr. Alpheus Smith, of the Quekett Club, exhibited specimens of the microscopic moth, *Cemiostoma laburnella*, and Salicine; Mr. Asbury Green, palpus of cardinal spider; Mr. Quick, foot of Dytiscus, toe of mouse, and head of gnat; Mr. Groves, a particularly good specimen of crystals of chlorate of potash; Mr. Burgess, section of yellow water-lily and group of young oysters; and Mr. Gay, the new Nudibranch (*Embletonia Grayii*); and the same in a larval state.

Mr. T. Charters White, of the Royal Microscopical Society, exhibited a young crab; Mr. W. T. Suffolk, lips of blowfly; Mr. J. Smith, Barbadoes Polycistina; and Dr. Millar, floreated spicules of Euplectella, *in situ*.

Mr. E. George, of the Forest Hill Microscopical Club, exhibited a specimen of the fructification of Chara; Mr. Martin Burgess, a foot of lady-bird; Mr. Burt, *Dicksonia Antarcitica*; Mr. J. R. Furneaux, parasite of canary; Mr. Thomas Rabbits, leg of diamond beetle; Mr. Goddard, Polycistina; Mr. E. F. Jones, a microscope of the last century, and dental plates of *Ophiocoma rosula*; Mr. Westbrook, larva of water newt, showing circulation of blood.

Amongst the makers of optical instruments were Mr. W. F. Stanley, of London Bridge, Messrs. Murray and Heath, Mr. James

Swift, Messrs. R. and J. Beck, and other celebrated makers, who exhibited instruments as perfect as the modern researches of science would permit, and some of the objects shown were very beautiful.

Taken altogether, the general opinion appeared to be that the exhibition of scientific objects was better than that of last year; and that greater interest is taken in scientific pursuits, owing to the establishment of the Croydon Microscopical Club, was evident from the increasing desire of the visitors to have the objects thoroughly explained to them; and it is only due to those gentlemen who exhibited, to state that they were untiring in their efforts to enlighten every thoughtful and anxious inquirer.

Microscopical Section of the Liverpool Medical Institution.*

The first meeting of this, the third session of the above Section, was held on October 20, 1871.

Mr. Hamilton in the chair, twenty-four members present.

This Section of the Liverpool Medical Society meets once in each month, and the subjects brought under its consideration are medical in character, or relate to medicine.

At this meeting the chairman urged each member of the Society to assist in prosecuting microscopical research, and added that the Council of the Society have voted a certain sum for that purpose, a room was being furnished in which members might work with the microscope and mount specimens.

Mr. Hamilton then read his paper on "The Identity of Life in its Earliest Manifestations."

The object of the paper, stated the author, is only tentative, a proposal, as the result of certain experiments to look for the origin of the earliest forms of life as a sequence of the disintegration of a compound life,—in a somewhat different manner than has hitherto been done. It is intended further to illustrate that the decomposition of organic matter, however varied in its kind, shows in the first stages of that decomposition similar forms (under the microscope) of rudimentary life. There is therefore a stage in the breaking up of all organic structures when they assume identical shapes.

The author's experiments consisted of two series. In the first of these, he placed portions of various kinds of animal and vegetable structures severally under conditions favourable for decomposition, and from the decomposition of each form of living thing he saw the same kind of animalcule spring. In the second class of experiments trial was made of solutions of carbolic acid, sulphurous acid, sulphate of copper, refined petroleum, chromic acid, and hydrocyanic acid, to prevent decomposition, and the author's conclusions concurred with those arrived at by Dr. Dougal, of Glasgow, and communicated to the last meeting of the British Association.

Dr. Braidwood next read his paper "On the Adulterations of Coffee, Pepper, &c., detectable by the Microscope."

* Received from Mr. Braidwood, Dec. 27, 1871.

An account was next given by Dr. De Zouche of a case of cerebral tumour (Glioma), of one of epithelial cancer of the rectum, and one of cancer of the stomach, sections of which were exhibited under several of the microscopes.

The members of the Society lastly proceeded to examine the specimens arranged under about twenty microscopes for their inspection.

The *second* meeting of this Section was held on November 17, 1871.

Mr. Hamilton in the chair, and eighteen members present.

At this meeting no paper was read, but descriptions were given of the specimens exhibited, and of the cases from which they were removed.

Dr. Carter narrated the case of a patient from whose body he had removed an extremely fatty liver, the cause of death being melæna.

Dr. Glynn exhibited a specimen of cirrhotic liver.

Mr. T. S. Walker showed sections of a melanotic tumour of the eyeball, in which the pigmentary deposit and the disintegration of the retinal tissue were well seen.

Mr. Banks exhibited sections of an adenoid tumour and of a tumour of nerve.

Dr. Braidwood showed portions of a very fine white powder which had become deposited on the inner surface of the glass of a locket enclosing a lock of hair from a child who had died a year previously from malignant scarlet fever. In the locket were placed, after the child's death, a lock of hair not disinfected (this was below a well-fitting glass cover) and a lock which had been disinfected with Condy's fluid (this was placed outside the glass cover and inside the lid of the locket). On the locket being opened a year later a fine white powder was seen on the inner surface of the glass, next to the *non*-disinfected lock of hair. When examined with a $\frac{1}{12}$-inch lens, this deposit was found to consist of very minute spherical granules (like the microzymes in vaccine lymph), and of sharply-defined, acicular, crystalline-like bodies.

Tunbridge Wells Microscopical Society.*

The October meeting of this Society was held at Rev. B. Whitelock's, when the subject of *Animalculæ* was considered. The meeting for the following month was held at the house of Dr. Johnson, the subject for discussion being *Sponges*.

* Report furnished by the Rev. B. Whitelock.

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